Seventh Annual Report

OF THE

AERONAUTICAL SOCIETY

OF

GREAT BRITAIN

FOR THE YEAR 1872.

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OF THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN,

FOR THE YEAR 1872.

Containing an Account of the Proceedings, and a Selection from the Papers and Communications* received by the Society during the year, with concluding Remarks upon the present state of the Science.

A General Meeting of the Members of this Society was held in the Theatre of the Society of Arts, John Street, Adelphi, on Tuesday evening, the 18th inst. Mr. JAMES GLAISHER, F.R.S., presided.

A new machine, constructed under the direction of the Society, for measuring the relation between the velocity and pressure of the wind, was exhibited.

At the request of the CHAIRMAN,

The minutes of the previous meeting were read by Mr. F. W. BREAREY, the Hon. Secretary.

* The Council, in publishing any paper which may have been read or communicated, disclaim any intention of endorsing the views of their respective Authors. It is with the belief that here and there a hint may be conveyed which may prove of use to those of the members who may be practically engaged in overcoming the acknowledged difficulties of the problem to be solved, that some of the papers have been published, which otherwise would appear hardly to justify their reproduction.

The CHAIRMAN: Ladies and Gentlemen,—the subject which will most naturally attract our attention this evening, is that of the experiments which have been made by the apparatus now on the table before us. I had almost forgotten that at our last meeting we spoke of this instrument having been designed. It was not completed so soon as we expected; and, although much time has been occupied in making experiments, the results are not quite so conclusive as could be desired; but so far as they go are important—not only in respect to the problem we wish to solve, but, as bearing upon the pressure of the wind on the surfaces of planes. I will not now engage your time longer, but I will ask Mr. Wenham, under whose care, in conjunction with Mr. Browning, the experiments were carried out, to give a statement respecting the results. It is an instrument of a kind which I have long desired, and it seems calculated to achieve what we require in this direction with greater accuracy than any other instrument I know. I call upon Mr. Wenham to explain the apparatus.

Mr. WENHAM expressed his regret at the absence of Mr. Browning, who had been associated with them in these experiments. To make this instrument understood, he would explain how it acted as an ordinary anemometer, for ascertaining the direct force of the wind on a plane, when in a vertical direction to its surface. This consists mainly of a vertical steel spindle, supported on a hardened steel centre. Through an eye at the upper end of the spindle, a horizontal arm passes, and is secured by a small cross-pin, which allows the arm to vibrate like the beam of a balance. The long end of the arm carries the planes; and the opposite short one has a sliding counter-weight, which is adjusted so as to exactly balance planes of different sizes at the long end of the arm. Each plane is clamped at the end of a tail rod, which is pivotted through the forked end of the arm, by a

vertical steel pin, as close to the plane as possible; the other end of the tail passes loosely through a vertical slot, slightly curved as a radius, from the balance centre of the arm. By this arrangement, the surface of the plane is always kept at right angles to the current, throughout the extent of its horizontal motion. A wooden shield is fixed close before the front of the arm, to protect this and the balance weight from the wind, so that the planes only may be exposed to its force. The action of the instrument, as a single anemometer only, or when the planes are set at right angles to the current of air, is obvious. The direct pressure is read off by the spring steel-yard, which is connected to the end of a lever from the vertical spindle, close to the base of the machine. In order to measure the vertical forces, the planes are set at the requisite angles from a divided sector, whose centre coincides with the clamping screw at the back. The raising force due from the various inclines, was read off by the upright spring steel-yard. It was found almost impossible for one observer to read off the horizontal and vertical forces simultaneously during fluctuations, therefore the readings were noted by two persons at a given signal—even this was a matter of some difficulty. The arrangement would be far more useful and perfect as a scientific machine, if fitted with a piece of clockwork, moving a paper cylinder, on which the vertical and direct forces would be simultaneously registered by separate pencils, describing two undulating lines, showing at a glance the relative forces; the experimenter would then have nothing else to attend to, but to see that all other conditions were acting properly.*

The CHAIRMAN: I think the remarks by Mr. Wenham important, especially with regard to the effects produced on the planes at different inclinations. When the plane

* The tabular statement of the experiments referred to, were published in the Annual Report for 1871.

was placed vertical, the pressure of the blast of air was direct, and tended only to move the plane in a horizontal direction—being that of the direction of the air itself—but when the plane was inclined, a part of the pressure was exerted in raising the plate in a vertical direction, and a part only in exerting a horizontal pressure; so that the latter was less than in the previous case. When the plane was placed at an angle of 45°, the horizontal force and the vertical force were found to be identical, as mentioned in the manner described by Mr. Wenham. It was also found that whether the exposed surface was a circle, a square, or a parallelogram, providing the area was the same, the results were identical to the degree of accuracy to which the readings could be determined. Anyone who had not considered with care the nature of the pressure produced by the flow or rush of a fluid, elastic or incompressible, against a plane surface placed in its course, might imagine that the system of parallel forces was merely equivalent to a single resultant force acting at the centre of pressure, and capable of resolution according to the ordinary parallelogram law. But this of course is not the case, for the particles of the fluid which come in contact with the plane, have somehow or other to get out of the way, by gliding along the surface of the plane (as they cannot get through it), and this produces a complication in the neighbourhood of the surface of such a kind as cannot be theoretically predicted. One thing, however, is quite clear, and that is, that the directions of all the small forces acting on the surface certainly are not parallel, and that we must therefore have recourse to experiment. Even the fact that when the inclination of the plane to the current (supposed moving horizontally) is 45°, the vertical and horizontal pressures are equal, is not by any means evident; nor in fact can it be *exactly* true; for supposing (to fix the ideas) that the upper part of the plane

is bent over so as to point in a direction opposed to that in which the current is moving, and making an angle of 45° with it, then most of the particles of air in the vicinity of the plane will, in order to get out of the way, be moving downwards along its surface; so that compounding this motion with that of the current, we should expect the horizontal force to be greater than the vertical. The experiments have shown that this difference is not appreciable to the extent to which the instrument can measure it. The same qualification also must be understood to apply to these results, from which it would appear that the pressure was independent of the form of the surface. The velocity of the current in these experiments, was measured by a Lind's Anemometer, an instrument that has never appeared to me to give very satisfactory results; but still the only one available for the purpose. I regret that the apparatus is considered by Mr. Browning to be too delicate to be used in the open air, but I hope that this will not be always found to be the case. As I have said before, difficulties exist only to be overcome, and some day I trust, we may obtain a series of experiments, in which ordinary wind will replace the use of the artificial current. I see Mr. Brooke present, who helped us with the experiments, and he may be able to say something as to the results gained.

Mr. Brooke said it was not exactly mentioned, but the fact was notorious to everyone acquainted with mechanics, that in whatever position the plane was placed, the horizontal pressure may be resolved into two—one perpendicular to the plane, the other in the direction of the plane. It was clear that the resolved pressure acting in the direction of the plane was wholly effective in raising the plane. The resolution of the pressure into two, was well known to everyone acquainted with the principles of mechanics; but it was to be understood that there were many other facts to be considered.

The simple geometrical consideration of the action of the pressure upon the plane, did not involve the necessity for the particles of air which had impinged upon the plane, getting out of the way to enable other particles to impinge upon it. This led, in this experiment, to a result which might have been expected, but which it was important to ascertain. There were two rectangular planes of the same shape and area, and one was capable of being inclined lengthwise, in relation to the wind, and the other crosswise. Supposing the wind to be coming in a given direction (indicated as being towards the speaker) it was quite clear, with the plane inclined lengthwise, there would be less surface of the plane impinged upon, than there would be in the transverse direction (indicated on the instrument). The particles which impinged upon the former, must move along the plane, and had much more difficulty in getting out of the way, than particles which impinged on the plane in the latter position. This would show that the effective pressure of the wind at the same velocity was greater upon the one plane than upon the other. And, conversely, a revolving, or oscillating plane, moving in the former direction (indicated), would move with less force than in the latter direction (indicated). And here was an illustration connected with the wings of birds, particularly of those that had powerful flight —where the wing was exceedingly long and narrow, it struck the wind in that direction (indicated). The experiment showed that from the same amount of surface, there would be greater effect upon the air by a long narrow wing, than by a short and broad one of the same area. That was one of the results that had been obtained by these experiments.

Mr. WENHAM: I partly neglected to show how this illustrates the flight of birds. You will find that the lifting power of the smallest angle is nearly five times that of the direct force. We were not able to try less angles. The smaller the angle of inclination, in regard to the current, the

less the direct force; and, comparatively, the lifting force is scarcely diminished. At 15 degrees, one force is nearly five times that of the other.

Mr. Harte asked if, in making those experiments, attempts were made to ascertain any pressure of the wind downwards.

Mr. Wenham: No! I omitted to mention that. A spirit level was laid across, so as to level the instrument. We had a trunk twelve feet long and eighteen inches square, to direct the current horizontally, and in a parallel course.

The Chairman: Certain conditions of current were tried by Lind's Anemometer.

Mr. Harte: Did you notice, in making these experiments, where the centre of pressure came?

Mr. Wenham: We were not able to ascertain very accurately. In all cases there was a tendency to lift the front edge.

Mr. Harte: Did you notice whether, according to the angle, the centre of pressure came forward?

Mr. Wenham: We found as the angle became more acute, the centre of pressure came nearer to the front edge.

Mr. Hall (of Acton): Was the experiment made with a surface larger than one foot?

The Chairman: We had one eighteen inches square.

Mr. Hall: A different result would, I think, be attained with two feet, from what was attained with one foot.

The Chairman: We have not spoken of two feet, because the shaft was scarcely large enough to give the even pressure required. We did not feel quite so certain with respect to large planes; and, therefore, the experiments with them are not included in these records; but I am ready to believe that the larger the planes, the larger the results. With areas of six inches, twelve inches, or two feet, the larger area, the larger are the relative results. I have had three or four anemometers together, and always found this to be the case.

Mr. BROOKE: I rise to make an explanation. The 0 in the return ought to be 90. It ought to be 15, 20, 45 and 90.

Mr. F. W. BREAREY (the Secretary): If there is any gentleman here who could give us any advantage with regard to a fan-blower, we should be glad to avail ourselves of it. The area was so small, that we could not expose much surface.

The CHAIRMAN: But we ought to give our thanks to Mr. Penn, for the blower he lent to us, and for the use of his steam power. The entire work of the shop was stopped, during part of the time we occupied it. I should like to ask you to thank Mr. Penn, for the facilities he gave us on that occasion for making these experiments. (Applause.)

Thanks were accorded to Mr. Penn by acclamation.

The CHAIRMAN: I have now to introduce to your notice a gentleman who, I believe, has travelled more than 100,000 miles, and has visited New Zealand, California, and many other parts of the globe. Wherever he has been, he has watched as much as possible the flight of birds, and, as the result of his observations, he thinks it possible for man also to obtain flight. He knows New Zealand as well as he knows London, and he is now about to give us the benefit of some of his observations. We shall, I am sure, be glad to receive them. (Hear, hear.)

Mr. HEAD (the gentleman referred to) read a paper on "*Flight.*"

"Flight is performed by birds, insects, mammals, and to some degree by fish; and long ago, in an old period of our world's history, by dragons.*

"Gliding down inclined planes is not true flight—because it must be very limited, and requires altitude in proportion

* Pterodactyls?

to length of horizontal distance. Still, animals possessed of the power of true flight, make great use of this advantage.

"Flight is performed in straight lines, and curved lines; and the curved lines may be of two kinds—the upward curved line of the flying-fish, and the downward curved line of the yellow-hammer and albatross. Bees, beetles, dragon-flies, cockchafers, and blue-flies fly straight—so do rooks, pigeons, ducks, and shags, and many other birds. Beetles, cockchafers, albatrosses, and ofttimes hawks, fly on aëroplanes, or under them rather, and are propelled in the desired direction, in the case of beetles and cockchafers, by their true wings blowing in the other.

"How albatrosses fly I do not exactly know. Weight acts on a flying-fish directly he leaves the water, and also his inability to keep up his speed; and so, by the law of continuity it describes an elongated curve, with some slight contortions, caused by working its aëroplane fins.

"A large flying-fish, about nine inches long, rose close to our weather-bow, and flew into a wave and rose again; it then flew nearly to windward a long way, and three times gave itself a fresh impetus by sculling its tail in the top of a swell; so that the fish was not lost to my view, though some distance off.

"A dragon-fly has two pair of movable wings, and can dart about backwards as well as forwards, and it can also be quite still seemingly on the air. Bats fly in a most erratic course, but that only proves what command they have over their powers of flight, and is not a sign of weakness. So that besides the true manner, there are two distinct modes of flight—one, with an aëroplane, as the albatross, beetle, and hawk—and the other without, as sparrows, flies, and bats.

"Flight, as it is performed by the non-aëroplane flyers, I leave, as being by far the most difficult to be achieved by us,

and as requiring not only an exact balance between the wings, but also an exact stroke equal to the weight of bird, and each wing equal to the other in direct line flight—the upstroke also being so regulated that the under-current of air is exactly equal to weight of bird—the down-stroke being the propelling blow, by blowing wind, or the air, in a direction the more opposite to the line of flight the better; as, if a fair blast blows ten pounds to the foot, and it be free in mid air, it would be driven in a contrary direction, with a force of ten pounds to the foot area of blast aperture, and if it presented five feet of area to atmospheric friction, it would be driven forwards at the rate of sixteen miles an hour.

"Sky-rockets fly straight, only perpendicularly to still water—in any other direction they are drawn—by their weight, or, as it is called, the alteration of gravitation, more and more down, till they strike the earth; but if they had the ears of a bat, or balancers like a fly, or a small fin aëroplane set to the exact angle, to suit their line of proposed flight, and the force employed, they would fly straight in any direction whatever—so long as the force was even and equal, and the wind did not vary; and, then, a sky-rocket is a flying machine of the cylinder blast kind, and for the time carries its power with it—but, as it continuously gets lighter, of course it could not be set to go quite true. A steam-engine might be made on the same plan, with one end of the cylinder out, but it requires a great supply of steam, and could not be made to go far.

And now I come to consider aëroplane flight, and the best mode by which it can be attempted by man; and—First, there must be three indispensable requisites, without which, it cannot be performed—weight; of which I need say no more, as that is easily obtained—but that without it, the machine might be any way up, and be carried about by any puff of wind. An aëroplane suspender, and a

force as a means to effect speed, or speed multiplied by depression of air, equals weight—for horizontal flight, and is so much greater for any angle upwards, to sixty degrees of altitude, above which an aëroplane could not be of much use. The great enigma is the necessary force to ensure a certain amount of speed to a plane surface, set to a certain angle, according to that speed, and with spread enough to drive down or press down air equal to the amount of weight of the whole machine and driver.

"Can it be found? that is the question. And what is the amount of force requisite?

"Suppose that he is drawn up an inclined plane of any angle, and that the friction is nil.

"On a level, it would require but a few pounds, except to start; scarcely anything in fact. At the angle of thirty degrees, it would require about one-half of half a horse, or one quarter; and twelve degrees, about half that, or one eighth. But we will reckon that a man and machine, at 200lbs., would require $400 \times 16 = 6400$ foot pounds per second—or rather more than $\frac{1}{5}$ horse power, to raise perpendicularly—that is, it would take one ordinary man and one-sixth of a man, to raise his own weight, and the balance of the 200lbs., sixteen feet, in the second of time, by any means at his disposal—without reckoning friction of atmosphere. To raise it twice as quick, would require four times the power, or nearly one-horse power perpendicularly—that is, at the rate of twenty-one miles an hour.

"But we do not want to fly straight up, nor above an angle of thirty degrees; which would not take quite half the powers of perpendicular ascent—excepting the friction which, on an aëroplane, you may count for nothing—(I am not now speaking of the friction of atmospheric resistance). And at an angle of ten degrees, about one man power would be sufficient to drive an aëroplane machine twenty miles an

hour; which, I consider, to be very well, for it would not often be required to mount even at that grade—and once up, one could easily go on level.

"I have not, however, taken into consideration the force required to drive the machine through the air.

"To drive one foot through the wind or air, at the rate of fourteen miles an hour, requires one pound of force; and, at the rate of twenty miles an hour, two-and-half pounds to the foot; and, reckoning the vertical area of the machine, man, and all at thirty feet, it would require one-fourteenth of a horse-power, or less than a half of a man to drive it through the air at the rate of twenty miles an hour.

"And now the conclusion is, that a man could not raise himself on a machine, by his own exertions, at a greater angle than about eight degrees of grade, and at no much greater speed than twenty miles an hour—but even if that can be done, it would not be a bad beginning.

"Flying will become a business, and not every one could attain to it; nor would it be desirable.

"But flight by steam will be achieved yet. An engine of 1-horse power could drive 1000 pounds up an incline of about 1 in 10, with proper appliances.

"Now for the appliances; calculating that a man—a gymnast at any rate—has power enough to sustain himself in horizontal flight an hour, and that he is the power obtained, and also the guiding will—having the weight and power—all we want is the aëroplane, and a means by which this power can exert itself in the air—in fact a machine, and one that can be kept in any proposed direction, and right way up.

"Before considering that, I should like you to direct your attention to an arrow, or to the long rod of a sky-rocket, or the tail of a peacock, or bird of paradise, and to the long body of dragon-fly.

"In the case of the arrow and sky-rocket, they are both kept in their places, or line of motion, by a light long drag

behind, as a steer-oar, and that is the best form for us to make use of at present, with weight forward.

"Twenty miles an hour is a low speed, and is reckoned to an angle of eight degrees, but horizontal flight could be maintained to many times that speed; and it entirely depends on the speed that can be obtained what size of aëroplane is required, and also the shape of it. The greater the spread the more air is passed over that has not been deflected downwards by the fore part of the aëroplane; and so, for the same reason, a wide one is useless, that is long in the line of flight. So an aëroplane, or rather the pair of aëroplane wings, must be long and narrow—say twenty feet each, and three feet wide, or even less—about two and a half, or two—rather more than the proportion of the wings of an albatross.

"Also, a short bevel in the line of motion is easily regulated by an aëroplane lever, while a large square surface is only so regulated with great difficulty.

"And now for the last consideration. How is the force to be applied?

"Whatever any one may say to the contrary all forward motion in flight is performed, or effected, by blowing in the other direction, and is the only way of doing it, as in swimming; and that can be done in many ways, as, a pair of bellows, a drum blast, or screw fans, or wing fans, or a fluke or common fa...

"A whale can nearly lift himself out of the water by the accumulated momentum gained by a fluke fan in the water.

"Now a fan blast can be constructed to blow, say one pound to the inch—but say 100lbs. to 1 foot, which gives a rate of 140 miles an hour to 1 foot, or about 25 miles an hour to 33 feet of exposed area of vertical surface.

"But as the machine would have to run on wheels in order to gain momentum to start, the handiest mode for us

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to make use of is to construct the pair of back blowing fan propellers of a size according to height and width of wheels, worked by a bevelled pinnace set to corresponding bevelled cog wheels inside the two driving wheels.

"So the whole machine may be described as a simple velocipede, with the two driving wheels in front, and of rather large size, held up by a supporting canopy over the driver (which is not absolutely necessary), and two narrow long wing aëroplanes, slightly elevated, and bent like an unstrung bow, and kept in direction by a long steer oar with a broad horizontal plane and a narrow vertical one.

"The steer oar to work on an universal joint, and the tiller to end in an oval ring, to encircle the aëronaut, allowing it also to work the small steering wheel aft.

"I wish now, in conclusion, to say a word or two concerning the albatross, because I consider that it is the best flier in the world. He always lives in a half a gale, the great Southern Cyclone, and round and round the Pole he glides on his way. He has been caught seventeen feet from tip to tip, and I am sorry to say that I never heard what one weighed, or his measured area of spread of wings; but I feel quite sure that as a pendulum takes but the slightest force to make it rise to the level from which it fell, so does the albatross fall from a height, and skim along and rise again to about a level with the point of departure, and so it flies on in, I think, parabolas downward, and with scarcely even a flap will keep with a ship travelling nearly three hundred miles a day, and coming and going for miles round all the time three thousand miles in a week."

In the course of the reading, Mr. Spencer exhibited a miniature model of a boomerang, which he discharged from a spring, exhibiting the gyrations of the instrument in the air, and its return to the point of discharge.

On the conclusion of the paper,

Mr. Moy, advancing to the black board, explained by the use of diagrams, the adaptability of Mr. Scott Russell's wave line to aërial machines. He said that he had studied ship-building in former years, and he thought that the knowledge so gained might be useful to aëronautical science. What they wanted in making the bow of a ship was to shunt the water off easily to the bend of the vessel. By using Scott Russell's wave-line, they found that they did no more work than they wanted. If they tried the same principle of the wave line by that instrument, they would find that whether the plane were long or short they got the same lifting power. Some gentlemen supposed that a cup surface would do better than that, but it would not. He should like to see an experiment tried on these planes if they could get a current of air blown upon them; at the same time it would be better if they could get it tried outside. It would be better if they could get the instrument attached to a railway-train going at great speed, that might serve their purpose more effectually than blowers. When they came to flying machines they wanted to get very fine angles. The coarsest angle he would like would be 5 degrees. At these fine angles the wave line curve was almost flat; but if they could accommodate those machines to the wave line it would be better than being quite flat. He mentioned this in order that those who were trying experiments might bear it in mind.

Mr. Harte would like to ask Mr. Head, who had had such a large experience, if he had observed any difference between birds when flying through calm air and when in a storm.

Mr. Head said that it was scarcely ever calm in the habitat of the albatross.

The Chairman said that would lead to the inference that the stronger the wind the more easily the bird moved through the air, and that the action would be different in a state of calm from what it would be in a gale.

Mr, Head was not sure that the albatross could fly save in a gale.

Mr. Wenham could not agree with the statement that a bird's weight can act as an abutment, or a persistent force, in helping to sustain it in one direction against the wind, like the string of a kite; or that the constant winds of the Southern Ocean are at all necessary to keep the albatross perpetually on the wing without effort. The bird is sustained by skimming over a vast body of air which may be in rapid movement against the earth, but with respect to its own condition is stationary. It may be a fifty-mile current, and if the bird make that speed in flight, in the direction from which the wind comes, it will make no progress relative to the ground, but in the opposite direction will speed on at the rate of 100 miles per hour; yet its progress through the body of air will be identical in both cases, or fifty miles per hour; and the conditions of flight are alike and the same as in still air. After the first abutment, spring, or momentum has been obtained, and the inertia from the earth expended, it ceases to exert any influence, and might be any distance off, or not there at all, as its presence does not affect the result. It was, therefore, a great mistake to suppose that the albatross was sustained in the air on account of currents prevailing in any one direction. The bird would exist in the same relation to the air as if it were in a calm, just as a balloon drifted along independently of the earth. It would be quite insensible of the current.

The Chairman: But the balloon goes with the wind—that I know to my cost (laughter). The bird goes against it.

Mr. Wenham: The bird with the wind will make 80 miles an hour; but, relatively, it would make the same, either one way or the other.

Mr. Head: I may make the remark that water in motion will carry big stones.

Mr. Wenham: There is a mistake. You throw a big stone into a rapid current, and it sinks to the bottom in a moment, and you will only see it bound and rebound as it is rolled along.

Mr. Head said that Canterbury Plains (New Zealand) were formed by boulders which had been brought down by the river. The whole of the West Coast of New Zealand was formed in the same way.

An Hon. Member: But those stones are at the bottom.

Mr. Moy: That is just the resistance of two elements. We might as well speak of a tile which was blown down in front of my house last January.

Mr. Stuart Harrison thought that with regard to that bird, the albatross, we had not got quite at the truth yet. The weight of the bird had, in his opinion, a great deal to do with the fact that it was sustained in the air. The weight of the bird served the same purpose as the string of a kite. Take the case of a balloon. The balloon had no gravity, no tendency to fall; but it simply floated as a piece of wood on water. Now take the case of the albatross. The wind impinged against the wing of the albatross, and, supposing that the bird had no gravity whatever, it was clear that the force of the wind on its wing would drive it more and more in one direction. The bird would continually rise; but the fact that the bird had gravity, enabled it to fly in another direction, at a fixed position, relatively to the earth. At that position the bird would remain over a fixed spot, with outstretched wing, because the current of air and the tendency of the bird to fall would counterbalance the elements of motion. Change of position would give that motion which the reader of the paper had so graphically described as a movement of the wing.

Major Robertson had seen many albatrosses fly, and quite agreed with the observations just made. With the

albatross, it was easier to fly in a gale of wind than a calm, because of its very great weight.

Mr. G. J. M. Hardingham was not quite satisfied about the albatross. Illustrating his remarks by lines and curves on the black board, he explained the action of the force of the wind, and the counter-balancing power of gravity as affecting the flight of the bird. But it was a mistake to investigate this albatross question so much. The action of a crow's wing would much more forcibly illustrate the action of a bird in flight. Instead of looking at extraordinary flyers, such as the albatross, they should take a simple flyer, and if they could explain that they could get at the other. A great deal of the different kinds of flight could be explained by the angular set of the wing—(explained on the board). Judging from his observations of the effect of the down strokes of the wing in sustaining a bird, he worked it out, and found that for a machine to lift one man the horse-power came to about twenty horse-power; so that the chief objection was, they would have to get a very strong man indeed to work a machine for the sake of lifting himself. The fact was, that the forward resistance was a mere bagatelle compared with overcoming the gravity. The great thing was, therefore, to overcome the gravity.

Mr. Hall remarked that birds of the crow kind very rarely soared about, or sailed with the wing in a motionless condition, as the albatross and birds of larger powers of flight. These larger birds brought their weight greatly into play to enable them to hold their own against opposing currents of wind. He believed, therefore, that when flight by human beings was brought into operation it would be by bringing the weight of the machine into play as a balancing power. It was weight that enabled the condor to fly many miles in a few minutes, without any motion of his wing, in the elevated region of the Andes. This could not be

obtained by any other means than weight. He had been brought to this view some years ago by observing gulls flying on the seashore. He noticed that they kept themselves suspended simply by bringing their weight into a state of equilibrium, and always keeping their head to the wind. He had formed many models upon this relation of weight and equilibrium. First, he formed them on the plan which Mr. Moy deprecated—the cup shape—but he found it better afterwards to adopt the wave-line stem for his embryonic flights. He was convinced that the wave-line was the right principle, and he was trying to bring it into action. His great difficulty was to get an opposing current of air strong enough to get the machine away from the earth. Dr. Pettigrew, in the current number of the *Natural Science Review*, had adopted the same view. He would ask the gentleman who read the paper, and who spoke of "flittering flight of bats," whether he had not seen large bats fly almost like the albatross, in a straight line.

Mr. Head: I have heard of them, but I never saw them.

Mr. Brooke thought there could be no doubt that a bird's wing did assume the wave-line in flight.

Mr. Arthur M. Saunders asked whether the wave-line was intended to increase or diminish the resistance. It appeared to him that with an aëroplane the object was to increase the resistance so as to give more lifting power.

Mr. Moy remarked that if they wanted to send a ship rapidly through the water they would adopt the wave-line; but if they wanted her to go down, they would adopt another shape. The wave-line got rid of the resistance forward.

An Hon. Member suggested that some experiments should be made on the wave-line principle.

The Chairman: That will be another class of experiments. The great thing was to connect pressure with velocity.

Mr. Hardingham would like to know how velocity was measured.

The Chairman: It was not measured. The experiment was merely by pressure on the surface. We have no idea at the present moment of the connection of pressure with velocity: it probably varies as the square. But I did hope we should have been able to get some results to-day.

Mr. Hardingham remarked that resistance was according to the sine. It was nearly the square of the sine.

The Chairman: There is one duty we have to perform. It is fortunate that this gentleman has been travelling in those regions so far and so long, and it is still more fortunate that he has come to give us, in the simplest language he has been able to use, the results of his observations; and I am sure you will thank him for what he has done (hear, hear). He has not only seen, but has reflected; and has put his thoughts into shape, and given them to us. I therefore ask you to thank him for his paper.

Carried by acclamation.

Mr. Head acknowledged the compliment.

The Chairman remarked that he had still a paper, written by Mr. Gosling, C.E., of Bombay, but it would be for the meeting to say whether at that late hour they would hear it, or would reserve it for a future meeting.

On the question being put, the latter course was approved, and, after passing a vote of thanks to the Chairman for presiding, the meeting adjourned.

THE paper published in the last Annual Report, containing extracts from "*Lectures on the Phenomena of Flight in the Animal Kingdom*," by M. Marey, of the College of France, was translated and contributed to the Society by Mr. T. J. Bennett.

A more detailed translation has been called for, in compliance with which we must almost absorb, if not exceed, the space allotted to the Annual Report for 1872 :—

THE MOVEMENTS OF THE WING OF INSECTS.

* * * * * *

We have begun to study the motions of the wings, and the first question which presents itself is the frequency of these motions. On this point direct observation is of little assistance ; the acoustic method, which consists in determining the frequency of the strokes of the wing by the pitch of the buzzing of the insect is more efficient, but we have seen that even the principle of this method has been contested, and that its application presents difficulties. The graphic method remains to be considered. This method consists in making the wings themselves record the strokes which they execute. When an insect is held in captivity by force which it cannot overcome, after trial it ceases a useless resistance ; it resigns itself and abstains from all efforts to escape, its wings remain immovable, and in this way the observer who hopes to study their motions finds himself disappointed. But there are different methods of awakening the insect to its original activity ; it is sometimes sufficient to pinch the antennæ lightly ; this irritation of a very sensitive organ succeeds with the *Macroglossa*. Among the wasps the end may be attained by titillating the feet, or by holding them all together with a pair of forceps, and then releasing them suddenly, except one, by which the animal is held. The captive supposes that it is at liberty, and makes an effort at flight, which lasts about thirty seconds, or long enough to be observed. There is, however, another difficulty. The captive insect, when willing, cannot fly like an insect at liberty, because the external conditions are not the same. It experiences a greater resistance in proportion to the traction which it exerts upon the bond which holds it ; to a free insect the relation is such as a boat held by an obstruction bears to one sailing freely, or as a horse which drags a load to one relieved from harness. This resistance modifies its behaviour considerably, and obliges us to distinguish between the two different conditions of free flight and flight in captivity. It is indispensable to establish these distinctions, in order to appreciate at their true value the results to which we are conducted by the graphic as well as the other methods which we may employ.

The apparatus on which the wings record their motions is the ordinary registering apparatus, consisting of a metal cylinder, covered with smoked paper, to which a uniform rate of motion is imparted by clockwork. Let us suppose that, instead of the motions of the wings,

we would simply register the oscillations of a vibrating-rod. For this purpose the extremity of the rod is furnished with a little style, which touches the blackened paper with its point, and, as the different parts of the movable cylinder pass successively before the point, the soot is detached from the places which it touches, and a trace produced. If the rod is not in vibration, it makes a long white rectilinear trace without sinuosities, a straight line which, rolled upon the cylinder, constitutes a circumference. If it is in vibratory motion, its trajectory will be a curved line, of which the sinuosities indicate all the circumstances of the motion, its phases of elevation, its depressions—in a word, all its movements—and consequently all the oscillations which the vibrating rod executes in space will be faithfully reproduced on the paper. If we would ascertain the frequency of the oscillations, it is sufficient to know the rate at which the cylinder revolves. Ordinarily a tuning-fork is employed, of which the number of vibrations is previously known, as, for example, one hundred vibrations per second. This is made to write its vibrations upon the registering cylinder below the line traced by the vibrating rod, of which the number of vibrations are desired. The comparison of the two tracings shows at once the number of the motions of the tuning-fork back and forth, that is to say how many hundredths of a second correspond to one oscillation of the rod; the number of motions of the vibrating body during a given time is thus known with great exactness.

It is not, however, as easy to obtain the tracing from the wing of an insect as from a vibrating rod, and this for several reasons. In the first place, it is very difficult to fix at the extremity of the wing a writing style; however light it may be, the rapidity of the motion to which it is submitted is sufficient in most cases to throw it off. If, however, after many trials and much precaution we are able to retain it in its place, a permanent cause of perturbation still exists from its very presence. Under the influence of this incumbrance the extent and frequency of the strokes of the wing are evidently diminished. It is easy to convince ourselves of this, by taking a *Macroglossa* and fixing it in the manner which we have previously described, that is, immovably between two strips of cork, by means of a pin. Looking down upon it, we perceive the extreme limits traversed by the wing above and below, which we have called the *dead-points*. If some substance is applied to the surface of the wing, we see by the effect of this burden, in diminishing the play of the organ, the two limits of oscillation approach one another, and the extreme upper position, which just now was almost vertical, inclines towards the horizontal. We may finally remark that it is only at the cost of considerable chafing against the surface of the moving cylinder that we can obtain a complete tracing of the movement of the wing. The wing cannot touch the cylinder, except during a very short instant of its stroke; that is, the instant when the wing reaches precisely the distance from the body of the animal to the cylindrical surface. The spherical figure which the margin of the wing describes in space, cannot have more than one point in common with the blackened cylinder. We can therefore only obtain, as the whole impression, a series of points at more or less regular intervals; and, if a more

prolonged contact is desired, it can only be by curving the wing and folding it upon itself, and consequently the natural curve which the organisation of the insect obliges it to traverse will be falsified and altered. In any case the friction against the blackened surface will retard the motion, and although the retardation which it causes may be neglected when it is opposed to bodies of large size, such as a tuning-fork or a vibrating-rod, it cannot be when the vibrating object is the delicate membrane which constitutes the wing of an insect. Again, the friction, although exceedingly small, is found fully comparable with the forces which come in play in the motion of the wing, and its intervention notably alters the action of the latter. Experiment has confirmed these views. In one case an insect executing the motions of flight, and rubbing its wings somewhat roughly against the paper, furnished 240 movements per second; by diminishing more and more the contact of the wing with the cylinder, there have been obtained 282, 305, and 321. If, therefore, we would have a faithful representation, it is necessary to renounce the idea of obtaining those beautiful, regular, and continuous lines which are produced by the tuning-fork or vibrating-rod, and content ourselves with interrupted lines, half-strokes, represented by fragments, or even only isolated dots, the periodical return in these incomplete markings of definite forms permits us to infer the repetition of similar oscillations, and hence to determine their frequency. The operation is as follows: with a delicate pair of forceps we hold the insect by the lower portion of its abdomen, in such a position that one of its wings at each movement shall lightly touch the blackened paper. Each of these touches takes off a portion of the soot which covers the paper, and, as the cylinder turns, new points incessantly present themselves to the contact of the wing. A figure is thus obtained formed of a series of points or short strokes of perfect regularity if the insect has been maintained in a fixed position.

We have obtained a large number of these tracings in which the wing has only touched the surface of the registering cylinder, and has left only a point as a mark in each of its vibrations. I exhibit a number of these, and trust as soon as the return of Spring permits us to procure insects to show you the experiments by which these tracings have been produced. Those which you are now examining have enabled me to determine the frequency of the strokes of the wings of the following insects:—

	Strokes per Second.
Common fly	330
Humble-bee	240
Honey-bee	190
Wasp	110
Sphinx moth (*Macroglossa*)	72
Dragon fly (*Libellula*)	28
Cabbage butterfly	9

Certain authors have estimated this number of vibrations by the acoustic method, but there is a notable discrepancy between the above figures and those which they have deduced from the pitch of the sound that these insects produce in flying. In the case of the common fly,

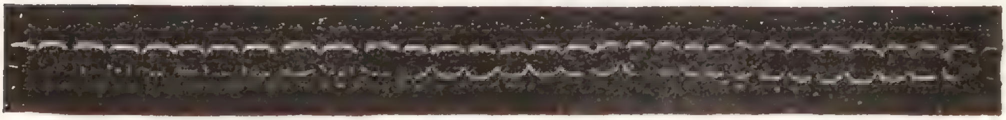
T. Lacordaire has computed the number of the vibrations of its wings at 600 per second, that is to say, twice as many as our figures exhibit. Has there not been a misunderstanding here, as is frequently the case, in the use of the word "vibration?" Some persons wrongly consider the raising and depressing of the wing as two vibrations, and reserve the term of "simple vibrations" for one or the other of these isolated motions. On the contrary, if we follow the usage most generally adopted, the two motions together, by which the body is again in its original position, should be considered as a single vibration.

The previous observations which we have made on free flight, and on flight under restraint, somewhat curtail the range which we are tempted to accord to these numbers. The animal, according as it desires to move with a greater or less rapidity, can change, at will, not only the extent of its wing-strokes, but also, to a certain extent, their frequency. Fatigue may exercise an analagous influence to that of the will; after very rapid motions, the exhausted animal diminishes the number of its strokes, which sometimes falls to a fourth or a fifth of its normal value. It continues to relax them more and more until a period of repose and reparation permits it to resume its usual flight; nevertheless, the examination of these numbers suggests some general considerations. We have reason to think that each of the muscular contractions which determine the drawing down of the wind is the result of a single impulse (*Zuckung* of the Germans), although in man contraction is due to successive impulses, which are merged in one another when they are produced more frequently than 30 times in a second. Among insects the limit of fusion of impulses is infinitely more remote, and ends with leaving the wing immovable, in a sort of permanent tetanic contraction. It is easy to assure ourselves of this by means of living insects, or better, by means of the artificial insect which I have constructed. When the impulses become too rapid, their extent diminishes; at this moment they no longer serve for the propulsion of the animal, whose wings appear quite immovable or merely agitated by a light tremor. Nevertheless, the number of muscular waves which the fibres of insects will admit without intermingling, a number which in the fly amounts to 300 per second, forms a physiological fact very interesting to note. Among other animals the limit is not so remote; among birds fusion is produced after 75 impulses; among mammals after 30, and among reptilia after only 4. These differences correspond, in virtue of the relations which I have long since explained to you, to analagous differences in the rapidity with which the elementary impulse traverses the muscular fibre of these different animals. The muscular fibre of the insect will then be characterised, physiologically, by the property which it possesses of furnishing a considerable number of distinct impulses, as well as it is anatomically characterized by its relative size and its striation.

The graphic process which enables us to judge of the frequency of the strokes, also permits us to show the perfect synchronism of the play of the wings. For this purpose it is necessary to choose an insect of which the amplitude of the wing-vibrations is large, so that in their moment of greatest elevation they may nearly meet above the dorsal region of the animal. If the insect is placed near enough to the regis-

tering cylinder, the dorsal region turned toward the blackened surface, it is clear that at the moment when the wings approach each other they will leave their traces on the paper, thus describing a series of loops and curves, of which the perfect correspondence proves the synchronism of the motions from which they originate.

Fig. 3.



Simultaneous tracings of the wings of a wasp in short flight. The perfect synchronism of the two wings will be observed.

Furthermore, we can convince ourselves that a sort of necessary connection exists between the motions of the two wings. If we throw an insect violently upon the ground, so that it is stunned and can no longer execute voluntary motions, we observe that, by producing motions in one of the wings, the other follows, to a certain extent, the injuries inflicted on its fellow. If one of the wings of an insect is depressed, the other also bends down; if one be raised, the other elevates itself. Certain species, especially the wasp, lend themselves very readily to this experiment. According to Chabrier, the author of an extensive work on the mechanism of the flight of insects, synchronism cannot fail to exist. This author considers the depression of the wing as the only effective portion of the stroke; its elevation is a passive phenomenon due to the action of physical forces. In fact, after the depression each dorsal arc of the thorax is deflected like a bent bow, and when the muscular contraction ceases the bow springs back in virtue of its elasticity, and the wing is raised. Now, if the pressure did not act simultaneously on the two extremities of the bow, it could not be flexed as it is, and the mechanism, which we suppose, would be impossible. The reality of this synchronism is, then, a strong proof in favour of this manner of understanding the motion of the wing.

After having determined, in a general manner, the frequency of the vibrations of the wing, we seek to know the variation produced in the number of these vibrations by agents capable of influencing the activity of the animal. In the first rank of such agents must be placed heat and cold. We know that warm dry weather is essential to insects, especially coleoptera, to enable them to fly well; special observation has confirmed this fact. We are able to state that, within certain limits, the frequency of the strokes is augmented with an increase of the temperature, and that they become slower under a gradual increase of cold.

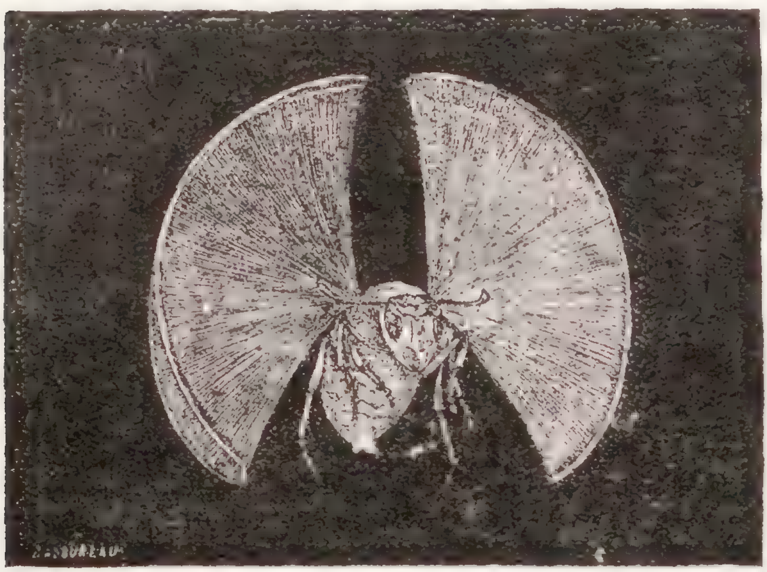
Form of the Motions of the Wings.

After having studied the frequency of the vibrations of the wings, it is necessary to study their form. For the end which we desire to obtain—that is, to arrive at a theory of the flight of insects—the most important element to comprehend is that which we now proceed to investigate, namely, the form of the trajectory which the wing describes in space, instead of the rapidity with which this trajectory

is described. In order to arrive at this determination we shall have recourse to two processes, which will reciprocally correct each other—the optic method, and the ordinary graphic method.

Optic determination of the movements of the wing.—When a brilliant body moves with rapidity, it leaves upon the retina a kind of luminous train, which acquaints us with the trajectory through which the body has passed. Children sometimes amuse themselves in producing the most varied figures by brandishing in the air a stick having one end on fire. It is on this principle that the apparatus, known in physics under the name of *Wheatstone's calidrophone,* is founded. This is a rod, fastened upright on a heavy foot, to which complex vibrations may be given, and to the ends of which a brilliant metallic bead has been affixed. If the rod is put into vibration the brilliant bead describes in space luminous figures, which vary with the different combinations of the vibratory motions. If a brilliant spangle can be attached to the extremity of the wing of an insect, this spangle, traversing without cessation the same points in space, leaves a continuous luminous figure exempt from the imperfection which is caused by friction in the case of the graphic cylinder. The extremity of an insect's wing can thus be rendered brilliant without mutilating it in any way ; it is sufficient to place upon it a drop of varnish, to which a small piece of gold-leaf is applied. The varnish dries so rapidly that the insect cannot throw off this little reflector of light, and nothing more is necessary than to hold the animal in a fixed position to observe the play of light upon the small brilliant surface. Under these conditions the bee and the wasp furnish a well-marked "figure of eight."

Fig. 4.



Aspect of a wasp, the extremity of whose primary wings has been gilded. The animal is supposed to be placed in a ray of light.

The figures of eight are more or less widened or compressed, according to circumstances. Sometimes the point of the wing seems to move almost in one plane. In the dragon-fly *(Libellula)* a figure of eight is also observed, but much more elongated; the loops are narrow and laterally compressed. With the *Macroglossa galium* it sometimes seems as if the preceding form had disappeared, and is replaced by a sort of ellipse. However, in examining it closely, it is soon perceived that this ellipse is surmounted by a little loop, very slightly developed relatively to the curve which supports it. It seems that one of the loops is enlarged at the expense of the other, but this last has not entirely disappeared, and the vestige what remains testifies to the persistence of the figure of eight which is encountered in most other cases, and which may serve as the general type.

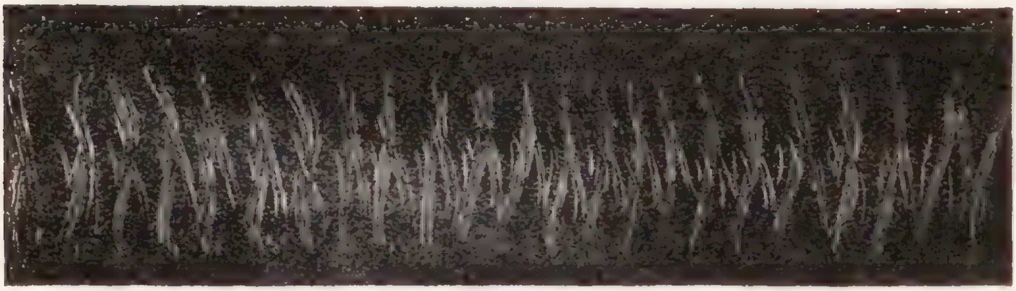
Changes of the plane of the wing.—The luminous figure which the gilded wing of an insect gives in its motions also shows that, during the alternate motions of flight, the plane of the wing changes its position in relation to the axis of the body of the insect. During the period of elevation the upper face of the wing is directed backward, while it turns a little forward during its descent. In fact, if we gild a large extent of the upper face of the wing of a wasp, taking care that the gilding shall be limited to this face, it is seen that the insect, placed in a ray of light, gives the figure of eight with a very unequal intensity on the two sides of the image, as is seen in the preceding figure. It is evident that the cause of this phenomenon is found in a change of the plane of the wing, a change in consequence of which the angle of incidence of the solar rays, while favourable during the ascent of the wing, is unfavourable during the descent. If the animal is turned so that the luminous figure is observed inversely, the figure of eight presents, in an inverse position, the striking inequality of its two halves, catching the light in a portion which was just before without it, and losing it where it had previously shone. We further find, in the employment of the graphic method, new proofs of the changes of plane in the wings of insects during flight. This change of plane is of great importance, for in this rests, as we shall see, the immediate cause of the propulsion of the body of the animal by the application of the motive force.

Method of contact.—Does the extremity of the wing really describe this double loop which we perceive, or is this form the result of an optical illusion—a play of flight? Though such an objection is hardly probable, it is necessary to refute it. To assure myself more entirely of the reality of the displacement of the wing than the optic method rendered perceptible, I have introduced, while the wing was in motion, the extremity of a little bodkin into the interior of the loops of the figure of eight, and I have established that in the interior of these curves free spaces really exist of a funnel shape, in which the bodkin penetrated without encountering the wing, while if I attempted to touch the intersection where the lines cross, the wing immediately struck against the bodkin, and flight was interrupted. Still greater precision can be brought to bear on the appreciation of these motions, and, knowing that the wing describes a double loop, it may also be

known in what manner it transverses the branches. It is sufficient to bring near to the wing in motion a leaf of paper blackened on both sides ; the wing, in pursuing its course, strikes against one of the sides of the paper, and the trace which it leaves testifies to the manner in which the motion is accomplished.

Graphic method.—This method is not applicable to our problem without important modifications. We have just seen that it is difficult to obtain tracings of any extent, because the wing cannot remain long in contact with the blackened cylinder, which it leaves and approaches successively. Under these special conditions it is necessary to have recourse to an artifice, and since it is impossible to obtain a satisfactory trace at a single stroke, we should try to divide the difficulty and separate the operation into several periods. The preceding experiments simplify the interpretation of the tracings very much, and we can reconstruct the figures which the optic method has indicated from the slender elements which they afford. I have considered in the complete course of the wing of an insect, such as is represented in Fig. 4, three distinct zones, of which I have obtained the tracings separately ; an inferior zone, corresponding to the lower portion of the figure of eight ; a median zone ; and a superior zone corresponding to the middle and upper parts of this figure. Bringing together the tracings obtained in these three zones, I have been able to reconstruct the entire curve. In registering the tracings of the median zone, figures much resembling each other are obtained, presenting the two crossed lines shown in Fig. 5.

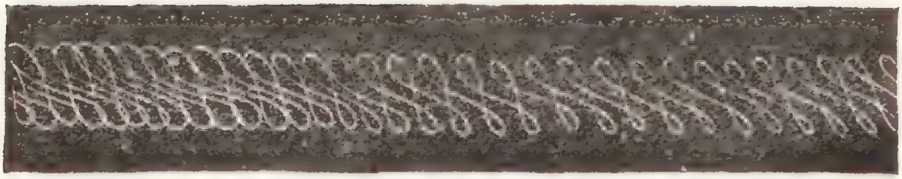
Fig. 5.



Trace of the median course of the wing of the *Macroglossa galium* (Bedstraw sphynx moth).

The multiple tracings of the figure are formed by the fringed extremity of the wing, which presents many small points. The upper portion is in the form of a loop, as well as the part which corresponds to the lower course of the wing, and these three parts successively obtained give, when united together, the complete representation of a figure of eight, such as is obtained in acoustics in registering by Kœnig's method the vibrations of a Wheatstone's octave rod ; that is, a rod which vibrates twice transversely for each longitudinal vibration. The slower motion of the cylinder produces the condensation of the end of the tracing.

Fig. 6.



Trace of a Wheatstone's octave rod.

The experiments can also be varied by obtaining, not the tracing of the point of the wing, but that of the anterior border of this membrane striking laterally against the cylinder. It is clear that in describing the upper loop, this edge will approach the cylinder, then deviating, in a similar manner it will describe the lower loop, so that in its complete course it will rub twice against the blackened surface, and leave two white traces separated by an interval. This is observed in Fig. 7.

Fig. 7.

This figure shows from the tracing of the wing of a wasp the upper loop and the whole extent of one of the branches of the figure of eight. The median portion of this branch is only dotted on account of the feeble friction of the wing. We may, therefore, be permitted to conclude that if the trace of an insect's wing could be obtained entire at one operation, the same figure would be presented which we have seen described in space by the gilded spot on the wing of the wasp, namely, a figure of eight, which our ingenious acoustician, Kœnig, was the first to obtain with a spiral Wheatstone's rod, making two horizontal to one vertical oscillation.

It now appears to me sufficiently established that in the more extended motions of flight the wings of insects describe a figure of eight in space. Furthermore, that the luminous figure which a speck of gold on a wing presents in its motions, has shown us that the periods of ascent and descent of the wing are accompanied by a change of plane in that organ. It is this fact which will shortly enable us to explain the mechanism of flight in insects.

Mechanism of the Flight of Insects—How they Propel Themselves.

The preceding lessons have been devoted to the study of the frequency and the form of the strokes of the wings of insects. You have seen that the frequency varied in different species, and in passing from the butterfly, for example, to the house fly, or the gnat, the variations may be considerable. The flight of the butterfly is slow, the strokes of its wings succeed each other at considerable intervals, propelling it by bounds and jerks, and producing an irregular and capricious flight. The gnat darts with rapidity straight at its object, emitting along its path a clear, sharp, strident sound. Between these two extremes we find all intermediate stages. Furthermore, the same insect, under different conditions, varies the rapidity of its motions within extensive limits; when free from all restraint its movements are rapid and precipitous, but when captured they are immediately relaxed, and although the frequency of the movements of the wing varies, the form of the motion does not change. It is in all cases the same, always a double loop, a figure of eight. Whether this figure be more or less apparent, whether its branches be more or less equal, matters little; it exists, and an attentive examination does not fail to reveal it.

Before drawing from this fact the conclusions which it warrants; before extracting from it the solution of the problem with which we are occupied—that is to say, the mechanism of flight—let us rapidly review the history of the question, and see how far previous authors have advanced in its solution. Without going further back, we find in the work of Borelli a chapter devoted to this subject, in which he considers the force which the bird or insect must employ to sustain or move itself in space. He estimates that this force is enormous; that it is, in the case of the bird, more than ten thousand times greater than the weight of its body. We still find this exaggeration in recent works. The academician, Navier, falls into an analogous error, and after him M. Babinet accords, in his turn, a power to the inhabitants of the air far superior to that with which they are gifted by nature. However, by the side of these errors we find a great number of correct ideas, since confirmed by observation. Borelli knew that the principal motion of the wings was an elevation and depression, executed in a vertical plane, and he asked himself how it was possible that this motion, which, it seemed to him, could only serve to elevate the animal or to depress it, should nevertheless contribute to onward motion. For this, it was necessary that the vertical force should be changed into a horizontal force. Examples of this transformation are frequent. If a wind blowing horizontally strikes against a flat board inclined forward at an angle of, say, forty-five degrees with the horizon, the action of the wind will tend to throw it backward and upward; or, if the board is moving forward with a momentum, it will tend to elevate it. We have here an illustration of a well-known principle of mechanics—the resolution of a single force by an inclined plane into two forces—which gives in part an explanation of the flight of insects and of water birds. But insects have four wings instead of two. Is the office of these four organs the

same; and if not, in what do they differ? Borelli does not treat of this question. It is discussed, however, in a particular case, by an anonymous author, who has left us an interesting manuscript on the habits of bees. This work, intended to complete and to correct the work of Réaumur, came from the Condamine Library, and belongs to M. Harnet. The author has observed bees at the moment when they hum at the mouth of the hive, trying to enter it and deposit their treasure. In examining the play of light on their trembling wings, he thinks that he saw the upper pair alone alternately raised and depressed, while the lower pair were animated only with a feeble horizontal motion. Here the question seems to have been abandoned, although the interest with which it is now regarded is far from inconsiderable. Beside the interest which it offers from the purely scientific point of view, in the mechanism of a function as widely employed as aërial locomotion, still another interest is attached to this study. The insect and the bird realize one of the oldest and most unsuccessful aspirations of the ambition of man. All space belongs to them; they go and come in the aërial ocean, while he is chained by his weight to the earth. Man has sought by various methods to escape from this confinement. The knowledge of the processes by which Nature attains the end to which he aspires, would perhaps have spared him many fruitless attempts and loss of much time and great waste of invention. In 1823 a work appeared in which this question of aërial locomotion is treated *ex professo,* and no longer in an incidental manner. The author, the Chevalier de Chabrier, studied the conditions of mobility of the wing, and arrived at the solution of an important question: how muscular action is transmitted to this movable organ. Is it directly, or by some intervention? The muscle, responds Chabrier, is not directly attached to the wing; it acts upon the arch of the back. When it contracts, the curvature of this arch is augmented; when it relaxes, the back returns to its original curve, like an unbent bow. In the motion of the wing, therefore, there is only one active period, the moment of depression; the period of elevation is passive. Elasticity, therefore, plays an important part in this function. Here, as in all mechanical organs, it absorbs and then gives out power; it regulates speed and produces continuity of motion.

But Chabrier was soon carried away by an exaggeration similar to that of Borelli and of Navier, though in a contrary direction. According to him, an insect needed an insignificant force for its propulsion in space. No effort was necessary to sustain it in the atmosphere; the animal floated there like an inflated balloon. In order to fly it filled its multitude of respiratory canals with air, and this, becoming heated, raised the animal as it elevates a hot-air balloon. It is not necessary to say that this conception of an *aërostatic insect* is an error. Without doubt an insect, before attempting a flight, lays in a quantity of air by a sudden respiration, but this provision of air contributes only an insignificant part toward the end which Chabrier assigned it.

The greater portion serves to prepare the organs of flight for the operation of their function. Jurine, of Geneva, in particular has

shown that the nervures of the wing membranes are small tubes which only acquire the rigidity and extension necessary to flight by inflation with air. We must refer to another contemporary, Strauss Durckeim, to find the elements of the theory to which my observations have conducted me. In his book on the Theology of Nature, a vast chaos of ingenious ideas, in which some profound, among many puerile, thoughts are to be found, there are many facts essential to the solution of our problem. Strauss Durckeim has conceived the ideal type of the insect-wing, the diagrammatic wing; that is to say, has reduced the organ to its essential parts. It consists of a rigid nervation or frame-work in front, a flexible web behind; this is all the apparatus. An apparatus thus constituted possesses the essential requisites for flight; otherwise constituted it will not serve this purpose, as is the case with the false-wing of the *Phryganidæ*, which has its principal nervation behind. It is enough that such a structure should be made to rise and fall successively: the forward border being rigid and the other flexible, it naturally disposes itself in an inclined position, receiving the reaction of the air obliquely, and thus transforms a part of the vertical impulse into a horizontal force. The two parts of the wing above mentioned are both indispensable in the same degree their respective offices complement each other in producing a single result. Ingenious experiments, due to M. Girard, throw light upon these facts. Destroy the anterior nervation, without removing the thin membrane, and the insect cannot fly; destroy the flexibility of the membrane by covering it with gum, and flight also becomes impossible. Here we cannot urge the objection that the superincumbent matter interferes by its weight like a burden which weighs down the animal; for, following out the experiment, we see that as soon as the coating becomes dry, small fissures are produced, flexibility reappears, and with it the possibility of flight returns. These observations assist us in comprehending the part which the anterior portion of the wings of the *Phryganidæ* play; which constitute the analogue of the stiff nervure, while the hinder wings represent the flexible membrane. The two wings of an insect thus complement each other.

I shall not further prolong this retrospect. I have limited it to the essential ideas entertained by our predecessors, and to those which will serve us in the future. The preceding experiments, joined to those which you have seen performed under your own eyes, seem to me to establish the following facts, namely: the motions executed by an insect during flight are limited to an elevation and a depression of the wings. It is true that other motions take place in the wings of insects. They are seen to move backward, and in repose to extend parallel to the axis of the body. We also see insects moving their wings backward and forward in preparation for flight. But these motions are not directly connected with aërial locomotion. The dragon-fly *(Libellula)*, which propels itself so rapidly, exhibits none of these lateral movements; its wings move exclusively in a vertical plane as if they turned on a hinge. But we have seen, in the optic method, that the course of the wing in space can be followed by gilding its extremity, and placing it in a ray of sunlight. Now this arrangement furnishes us with a figure of eight,

and we further know that during each complete vibration the wing changes its inclination twice. These movements are not controlled directly by the muscles. They are the mechanical effects of the resistance of the air acting alternately on the upper and lower surfaces of the wing in its alternate movements. When the wing leaves the upper limit of its position it inclines neither to one side nor to the other, its plane being parallel to the length of the animal. But when the impulse of the air is exercised, or as soon as the wing begins to be depressed, the rigid portion, the anterior nervation resists flexure while the flexible membrane which follows it gives way; drawn down by the nervation which lowers it, elevated by the air which uplifts it, this membrane takes an intermediate position; it inclines about 45 degrees, more or less, according to circumstances. The wing continues its downward motion thus inclined toward the horizon. Thus the reaction of the air, which combines its effect and acts perpendicularly upon the surface which it strikes, can be decomposed into two forces, a vertical and a horizontal force; one serving to elevate and the second to propel the animal. After this first period the wing membrane will have arrived at the end of its course; the direction of its motion is changed, its action is reversed. A moment of repose, infinitely short, separates these two phases during which the wing resumes its normal position parallel to the axis of the body. The nervure draws it up again, the air resists as before, and from this conflict results a position between the horizontal and the vertical—an inclination of 45 degrees. This second period contributes as did the first, to locomotion. How remarkable is the simplicity of apparatus by which the desired end is attained!

The horizontal force which is generated by the inclination of the plane of the wing is transmitted to the body of the animal and helps to push it forward. But as the body of the insect does not instantaneously take up the motion which is imparted to it, a part of this force is expended in curving the nervure of the wing which, at the same time that it is lowered, is pushed forward. Here is an artifical wing of large size constructed in accordance with the type which we have described; an anterior nervation represented by a stiff rod, with a membrane behind formed of paper pasted upon its edge. Try to strike down an object immediately before you, and you will not succeed. If you strike at an object before you with a downward blow the wing will be resisted by the air, and it will deviate greatly from the point at which you are aiming. From this deviating motion of the wing from the change of plane which it effects, the looped figure which it describes evidently results. It is the combination of these motions which generates the figure of eight previously described. We can now safely say that the two experimental facts are now interpreted by our theory.

A very slight difference has been observed between the two sides of the wing in certain insects; the lower surface is less polished than the upper; it is furnished with rugosities, hairs, or points, which according to Chabrier, give more hold on the air and reduce the loss of force by sliding. This disposition may contribute to insure the predominance of the useful effect of the lowering over the elevating motion. Furthermore, this predominance of the depressing action of the wing does not exist in all insects. These find that force as well in the period of

elevation of the wing as in the period of its depression, turning almost horizontally the plane in which their wings move. The numerous varieties which the mechanism of flight presents among the species of insects which we have observed will be studied later; they do not conflict with the fundamental principles which I have just announced.

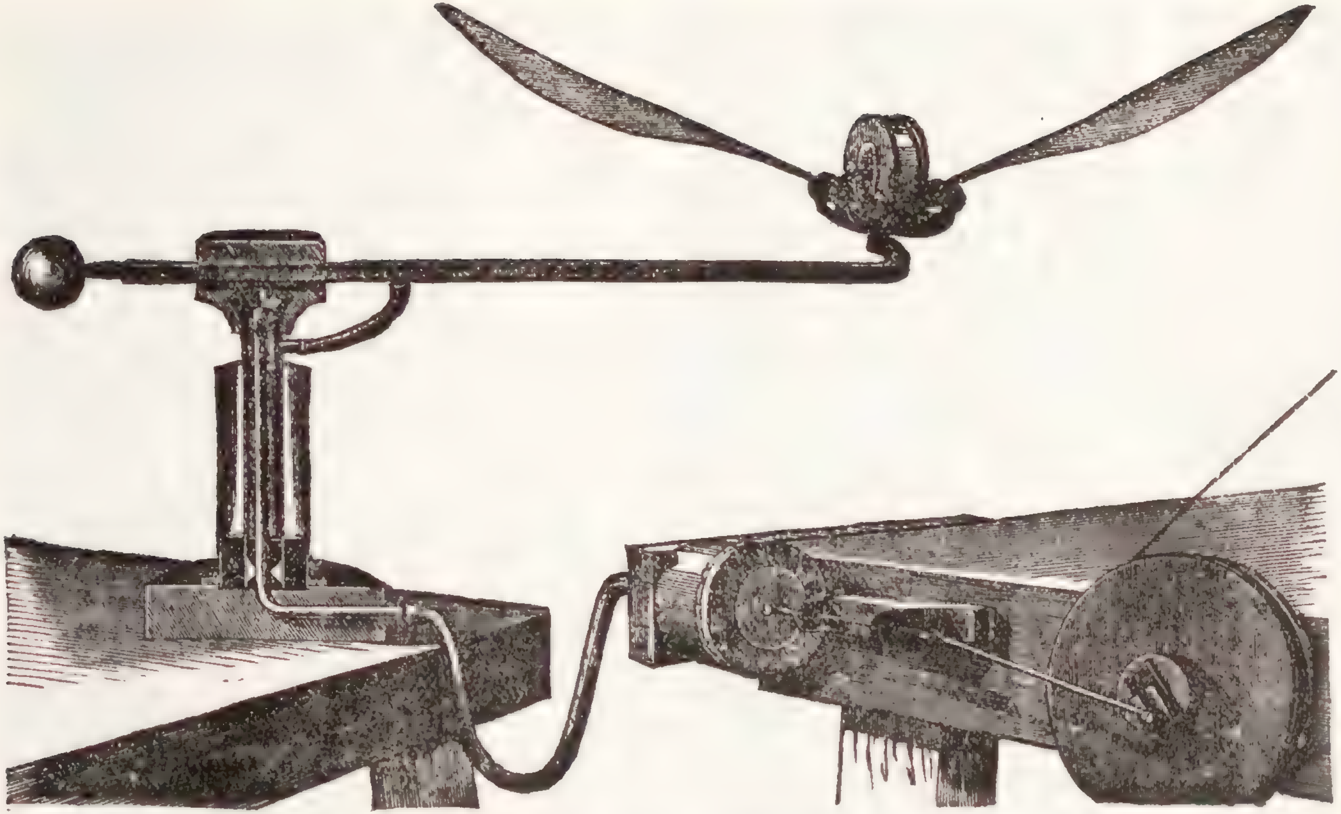
The mechanical conditions which we have just passed in review I have realized in a theoretical apparatus, from which I have obtained the same results as afforded by living insects. This artificial insect is represented by Fig. 8.

An air-pump, moved by a rotary apparatus, alternately compresses and relaxes the air in a tube which traverses the central pivot of the apparatus, where a sort of mercurial gasometer hermetically seals it while permitting the free rotation of the arms. The horizontal branch is hollow, and conducts the air into the apparatus, which is closed by a hollow metallic drum, of which the two circular faces are closed by two sheets of rubber. By the play of the air-pump these two sheets are inflated or contracted both together. They communicate the rapid motions of elevation or depression to the wings by two angular levers. The wings presenting, like those of an insect, conditions of unequal flexibility, decompose the resistance of the air, and impart to the apparatus a rapid rotary motion around the central pivot.

Imagine two artificial wings, as nearly alike as possible, both inserted on one of these little drums, which I have frequently described. They receive through this drum absolutely synchronous motions of elevation and depression. This apparatus is fixed at the extremity of an arm balanced by a counterpoise, and turning upon a pivot. This arm is hollow, furnishing a canal by which the effect of inflation can be transmitted to the movable drum of the wings. We may consider the drum as representing the body of the insect, and nothing prevents us from really giving it the shape of this animal. The rigid nervures, furnished with flexible membranes disposed to the right and left, will be the two wings, and the animal, instead of being free, will be fixed at the extremity of a movable rod; there is, therefore, only a single motion possible, which is that of turning around the pivot, carrying the attached rod with it. In effect, if I put the air-pump in motion, the artificial insect moves, flaps its wings, and really flies. At each stroke there is a change of plane of the alar membrane; at each stroke the point of the wing describes a figure of eight; and in a general way this theoretical animal, this artificial insect, reproduces all the particulars which the observation of real insects has revealed to us.

This apparatus affords many other advantages besides those of verifying theoretic ideas. It enables us to make new experiments, to which living beings will not lend themselves. We can change one of the conditions, for example, the form of the wings, their extent, or the rapidity of the stroke, or any other of the circumstances, while all the others remain constant; we may thus discover the influence which each of them singly may have on the mechanism of flight. It is by such experiments that we can assure ourselves of the following fact. In the course traversed by the wing there is only one region useful in the propulsion of the insect; that is the median region. In the two extreme portions the wing has not experienced that change of plane which renders

Fig. 8.



Representing the artificial insect or scheme of the flight of insects.

its action effective. Thus we see if we diminish the extent of the motions of the wing, the tractile power produced by the apparatus diminishes considerably, and finally ceases altogether. If the membrane of the wing is too broad, another phenomenon results. The hinder edge of the wing remains almost immovable in space, especially during motions of small amplitude; the nervure only is animated with rapid motion. The air, therefore, is struck by planes inclined inversely to those which act upon it in normal flight, so that the apparatus retrogrades and turns around its pivot in a direction contrary to its usual motion.

Experimental flight also shows the adaptation of certain forms of wings to obtain the most rapid translation of motive force. These are precisely the forms which we find in nature. The nervure of insects does not carry the wing membrane back to its point of insertion. Those parts near the articulation have little vitality; they contribute very little toward a useful result, embarassing the neighbouring parts, without compensation of any kind. The membrane should not exist except when vitality itself exists in a corresponding degree. Finally, the extent which the alar membrane should have, to best utilize the disposable force, can be determined experimentally. M. de Lucy has compared, in the case of a certain number of animals, the surfaces of the wings to the total weight of the body. He finds an extent of 30 square millimetres in a gnat weighing 3 milligrammes; 1,663 square millimetres in a butterfly weighing 20 centigrammes; 750 square centimetres in a pigeon weighing 290 grammes; 4,506 square centimetres in a stork weighing 2,265 grammes; 8,543 square centimetres in an Australian crane, weighing 9,500 grammes. But to facilitate the comparison it is necessary to reduce these figures to a common measure; and in spite of the barbarous phrases to which they lead us, we obtain:

	Square metres.
The kilogramme of the gnat represents	10·0
The kilogramme of the butterfly represents	8·0
The kilogramme of the pigeon represents	2·586
The kilogramme of the stork represents	1·988
The kilogramme of the Australian crane represents ..	0·899

The extent of the wings, therefore, is not proportionate to the size of the animal. A wing being given, a maximum rapidity of stroke corresponds to it. To augment the rapidity of the stroke, in hope of indefinitely accelerating the rate of flight, would be illusory; it is possible to accelerate it up to a certain point, but beyond this maximum limit additions become useless. Increasing progressively the action of the air-pump, the strokes of the wings are more rapid, and at first the rapidity of flight will be augmented. Continue the increase, and the rate of flight diminishes. The amplitude of the motion also experiences a considerable reduction, so that at the limit the wings appear motionless, or animated only by a slight quivering. Passing this extreme limit, the apparatus retrogrades. A given wing then corresponds to a fixed rate of progressive strokes; for, by the effect of inertia, the frequency of the strokes is increased only at the expense of their extent, and, when the extent diminished, the propelling force diminishes with it. I leave to yourselves the task of explaining these facts, which are the simple

consequences of the principles I have previously explained. I also leave to you the comparison of the mode of progression of insects with the other modes which are seen in other animals or in various mechanical contrivances. You will discover almost everywhere the mechanism of the revolution of forces on the principle of the inclined plane. You will find it in the motion of the tail of a fish, the principal organ of its locomotion; in the sculling motion of a waterman's oar, and even in the screw of a steam propeller.

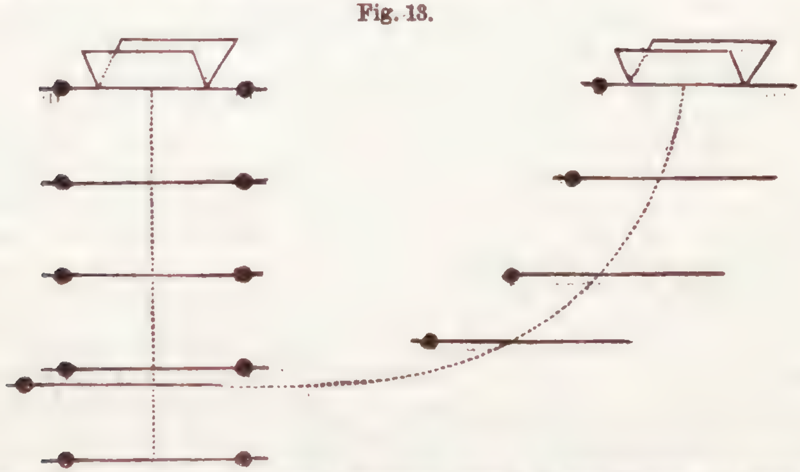
FLIGHT OF BIRDS.

By the simple inspection of a bird's wing it is easily seen that its mechanism for flight is not the same as that of an insect. Let the manner in which the feathers of birds are laid, one over another, be observed, and it will be evident that the air resists the motion of the wing only from below, so that in an inverse direction it finds an easy passage between the long beards of the feathers, which, in this motion, are no longer pressed together. This well-known arrangement, the effect of which Prechlt* has clearly pointed out, has led to the belief that to sustain the bird against gravitation the wing needs only to oscillate in a vertical plane, in consequence of the predominance of the resistance of the air acting from below over that acting conversely.

* * * * * *

All thin curved bodies tend to slide upon the air in the direction of the radius of their special curve. If we bend the anterior or posterior edge of our little apparatus at a certain point in its oblique course, we shall see it rise, notwithstanding the force of gravity, though its motion soon ceases. What has happened in this case?

Fig. 13.



Representing to the left Pline's apparatus placed in equilibrium by means of two equal balls at the extremities of the rod which lies at the bottom of the angle of the bent paper. This, as is indicated by the lower representations of the rod, falls vertically. To the right the same apparatus, with only a single ball, is represented. It descends in a parabolic curve, represented by the dotted line.

* Untersuchungen über den Flug der Vögel. 8vo. Vienna, 1846.

When there has been but little rapidity in the fall of the object, the curve of its surface remains motionless, because the air offers resistance only in proportion to the rapidity with which they move. Therefore, when this rapidity has been sufficiently great a steering effect is produced, which elevates the anterior extremity of the object and imparts an ascending motion to it. But very soon the weight, which was the motive

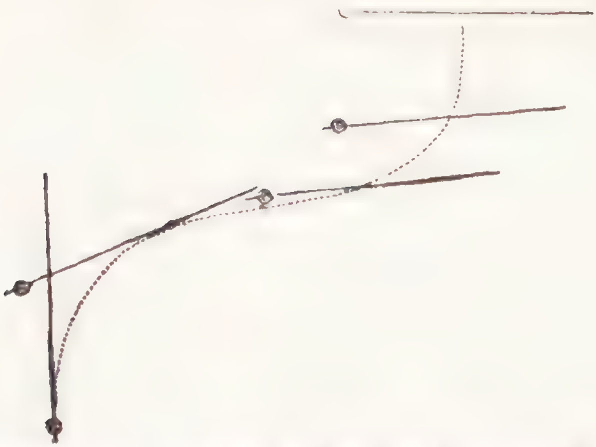
Fig. 14.

The posterior corners of the two planes of the apparatus have been bent upward and inward, so that after a descending curve the apparatus rises, as the dotted line indicates.

power of the apparatus, becomes a retarding force, and in proportion as the object ascends its motion becomes slower, and finally ceases. After this, retrogradation begins, to be followed by another rise, and so on, until by successive oscillations the apparatus finally reaches the earth. I may add that if a slight concavity is given to the object below, the reverse takes place, and we see at a certain moment the trajectory sharply deflected downward, and the object strikes the earth with great violence. In the second case, at the moment when the steering effect is produced, the weight is in a favourable position for a precipitate descent, and opposed to the ascending reaction.

I emphasize these effects because they are frequently produced in the flight of birds. The old treatises on falconry describe the interesting evolutions of the birds employed in hunting. Without going back further, we find in Huber (octavo, published at Geneva in 1784) a description of the curvilinear movements of the falcon, to which they gave the name of *passades*, and which consisted in an oblique descent of the bird, followed by a rise in its course. "The bird," says Huber, "when about to strike the earth, carried away by its own rapidity, would be dashed to pieces if it did not call into action a certain faculty, which it possesses, stronger than its descending motion, to rise even high enough to make a second swoop. This motion is sufficient, not only to arrest its descent, but even to carry it without effort as high as the elevation from which it came."

Fig. 15.



The posterior corners of the paper have been bent downward. After passing through a parabolic curve the object takes a very rapid descending course.

There is certainly exaggeration in the statement that the bird remounts as high as the elevation from which it descended without further effort. The resistance of the air must overcome part of the force acquired during the descent, and which is transformed into ascending force. We see, however, that the phenomena above described is confirmed by observation, and that it has been considered in some sort as a passive act in which the bird expends no muscular power. The act of hovering in some cases presents a great analogy with the phenomena just described. When some birds, pigeons for instance, have used their wings during a certain distance, the wings are seen to be perfectly quiet during a few seconds gliding through the air, either horizontally or rising or falling. The descending motion has the longest duration; in fact, it is only an extremely prolonged descent in which motion is maintained by the force of gravity, which diminishes it in the horizontal or ascending plane. In these latter forms the wing, more or less obliquely directed, takes hold on the air like the toy kite, with this difference, that motion is imparted to this by pulling the string when the air is calm, while the bird utilizes momentum previously acquired by an oblique descent or previous strokes of the wings.

I have already said that observers have admitted that certain birds, which they call sailors, can sustain and direct themselves in the air by means of the wind alone. This theory appears paradoxical. It is incomprehensible that a bird, motionless in the wind, should not yield to the resistance of the air through which it glides. If the *passades* or swoops which the falcon executes can sometimes carry it against the wind, this can only be a transient effect, compensated for by being carried away by the wind more rapidly in another moment. However, this theory has been sustained with great talent by some observers, especially the

Count d'Esterno, the author of a remarkable memoir on the flight of birds. "Every one," he says, "can see some birds practising this method of flight; to deny it is to deny self-evident facts." I myself have noticed this mode of flying, but it has seemed to me that it is executed in general under the following special conditions: Along the cliffs of the coast of Normandy I have seen the gulls and sea-mews performing their evolutions without moving their wings. I have seen the daws and rooks flying in the same manner around old cathedrals. But the same birds, when they left these special stations, have always appeared to me to use the rowing method of flight; that is to say, making regular strokes of their wings, sometimes interrupted in the daws by swoops of short duration. I then sought to determine the direction of the wind, and this is what seemed to me to occur: When a bird finds itself in the neighbourhood of a cliff, where the air is calm or agitated by eddies in a contrary direction to the prevailing wind, it can pass successively from the calm to the agitated air, and conversely. A sea-mew surrendering itself to the force of the wind, receives an impulse which carries it with a certain rapidity, and if, by simply turning, the bird enters a region of calm air, it can utilize the impulse which the wind has given it in returning to the height which it had left. Plunging again into the zone of agitated air, it recommences the evolution which I have just described, without moving its wings, except to give them different inclinations. The daws and rooks appear to me to find the same conditions around the cathedral towers. The authors who have reported the most curious cases of sailing flight have observed them in mountainous regions. It is a condor in the Cordilleras, or an eagle in the Pyrenees. The sailing flight has often been described of certain birds of prey, who, in the middle of a plain, rise and turn without moving their wings. I myself have often seen harriers fly in this manner, but I have always determined, also, that in this case the spiral which they describe is altered by the wind, and that the birds are definitely carried to leeward with a more or less rapid motion.

Even when reduced to these limits the influence of the wind on the flight of birds is very difficult to explain. It is complicated by very different conditions in which the motion acquired by the bird, opposed from various directions by the force of the wind, gives rise to the most varied combinations of motion. It is also known that in the upper regions of the air various currents exist, sometimes even in a contrary direction to those which obtain near the surface of the earth, so that the bird, passing from one to another, finds forces which carry it in opposite directions.*

Finally, the question of sailing flight seems to me one of the most difficult to solve. It would be temeritous to absolutely condemn the opinion of observers upon such vague theories and ideas as we possess upon the subject.

One of the most interesting points in the conformation of birds

* The late Mr. Espy suggested that the phenomenon of sailing in the flight of birds is due to upward currents of air which take place in warm weather, or beneath clouds, and especially up the side of a mountain against which the wind is blowing.—J. H.

consists in the determination of the relations of the extent of the alar surfaces to the weight of the animal. Is there a constant relation between the weight and these surfaces? This question has been the cause of numerous controversies. It has been already shown that if birds of very different kinds, yet of the same weight, be compared, the wings of some species are found to have four or five times the extent of others. The birds which have large wings are usually those which have been called "sailors," while those which have the wing short and narrow are generally classed as "rowers." But if we compare two "rowing" birds with two "sailing" birds; if, for still closer comparison, we take them from the same family, in order that the only differences shall be those of form, a somewhat constant relation will be found between the weight of the bird and the surface of its wings. But the determination of this relation should be based upon certain considerations, which have long escaped the attention of naturalists. Mr. de Lucy sought to measure the surface of the wings and the weight of the body in all flying animals. Now, to establish a common unit among animals of such different kinds and forms, he reduced all the measures to an ideal type, of which the weight should always be one kilogramme. Thus, after having proved that the gnat, which weighs three milligrammes, possessed wings with a surface thirty millimetres square, he concluded, in the types represented by the gnat, the kilogramme of animal was supported by an alar surface of ten square millimetres. By making a comparative table of the measures taken from a great number of animals of different kinds and various forms, he arrived at the following figures:—

Species.	Weight.	Wing surface.	Surface per kilogramme.
Gnat	3 milligrammes...	30 sq. millimetres ...	10 sq. millimetres
Butterfly	20 centigrammes	1,663 sq. millimetres..	8¼ sq. millimetres
Pigeon	290 grammes	750 sq. centigrammes	2,586 sq. centimetres
Stork	2,265 grammes ...	4,506 sq. centimetres	1,998 sq. centimetres
Australian crane	9,500 grammes ...	8,543 sq. centimetres	899 sq. centimetres

From these measurements, in spite of variations in detail, the evident result is obtained, that animals of large size and great weight sustain themselves with a much smaller proportional alar surface than smaller animals. A similar result already shows that the office of the wing in flight is not merely passive, for a sail or parachute should always have a surface proportioned to the weight which acts upon it; considered, on the contrary, from its true point of view, that is to say, as an instrument for striking the air, the wing of the bird should, as we shall see, present a relatively smaller surface in birds of large size and great weight. The astonishment exhibited at the result of the determinations made by Mr. de Lucy disappeared when it was remembered that there was a geometrical reason why the alar surface could not increase in proportion to the weight of the bird. In fact, if we take two objects of the same shape, two cubes, for example, of which one shall be twice as large in diameter as the other, each one of the faces of the larger cube

will be four times as large as the corresponding face of the smaller, while the weight of the greater cube will be eight times that of the lesser one. For all similar geometrical solids, the linear dimensions having a stated relation to each other, the surfaces are as the square and the weight as the cube of their similar linear dimensions. Two birds of similar form, but having, one of them, the spread of the wings from tip to tip twice as great as in the other, will have respective wing surfaces in the proportion of 1 : 4, and weight as 1 : 8. M. P. Demondésir, who applied these principles before me, thought that he had found in them a reason for the smaller size of birds being capable of flight, while those of a larger kind, such as ostriches and cassowaries, do not fly; he observes that if these birds had as large wings as the heron in proportion to their weight, they could not fold them completely, and would drag them as long and embarrassing appendages. These observations would be correct according to the theory of "sailing" flight, but, in "rowing" flight, the amplitude of the stroke of the wing, increasing in proportion to the size of the bird, multiplies the resistance which the wing meets from the air, and the reaction bears a similar proportion to the weight of the birds themselves. Dr. Hureau de Villeneuve, upon the same principle, has sought to determine the alar extent which would enable a bat of the same weight as a man to fly. He found that each of its wings would be less than three metres in length.

A remarkable work by Hastings* has appeared this year on the relative extent of the wings and the weight of the pectoral muscles in the different species of flying vertebrate animals. The author first shows that among birds the existence can be established of a certain relation between the surface of the wings and the weight of the body. But we should be careful to compare only comparable elements; that is to say, the length of the wings, the square root of the alar surfaces, and the cube roots of the weight among different birds. Let l be the length of the wing, a its area, and w the weight of the body, we can compare among themselves l, $\sqrt{a}$, $\sqrt[3]{w}$.

Examining different types of birds, Hastings made weights and measurements, from which the following table is extracted:—

Species.	Weight.	Surface.	Relation between them.
	w.	a.	$\sqrt{a}\ \sqrt[3]{w} \div \sqrt[3]{w}$.
Laurus argentatus	565·0	541	2·82
Anas nyroca	508·0	321	2·26
Fulica atra	495·0	262	2·05
Nettion crecca	275·5	144	1·84
Laus ridibundus	197·0	331	3·13
Machetes pugnax	190·0	164	2·23
Rallus aquaticus	170·5	101	1·81
Turdus pilaris	103·4	101	2·14
Turdus merula	88·8	106	2·31
Sturnus vulgaris	86·4	85	2·09
Bombycilla garrula	60·0	44	1·69
Alauda arvensis	32·2	75	2·69
Parus major	14·5	31	2·29
Fringilla spinus	10·1	25	2·33
Parus cæruleus	9·1	24	2·34

* Archives Neërlandaises, t. iv. 1869.

The weight of the pectoral muscles is, on the contrary, in simple proportion to the total weight of the bird, and in spite of the differences which correspond to the different degrees of aptitude to flight with which each species is endowed, we perceive that the proportion of the weight of the pectoral to the total weight is about one-sixth in the greater number of birds.

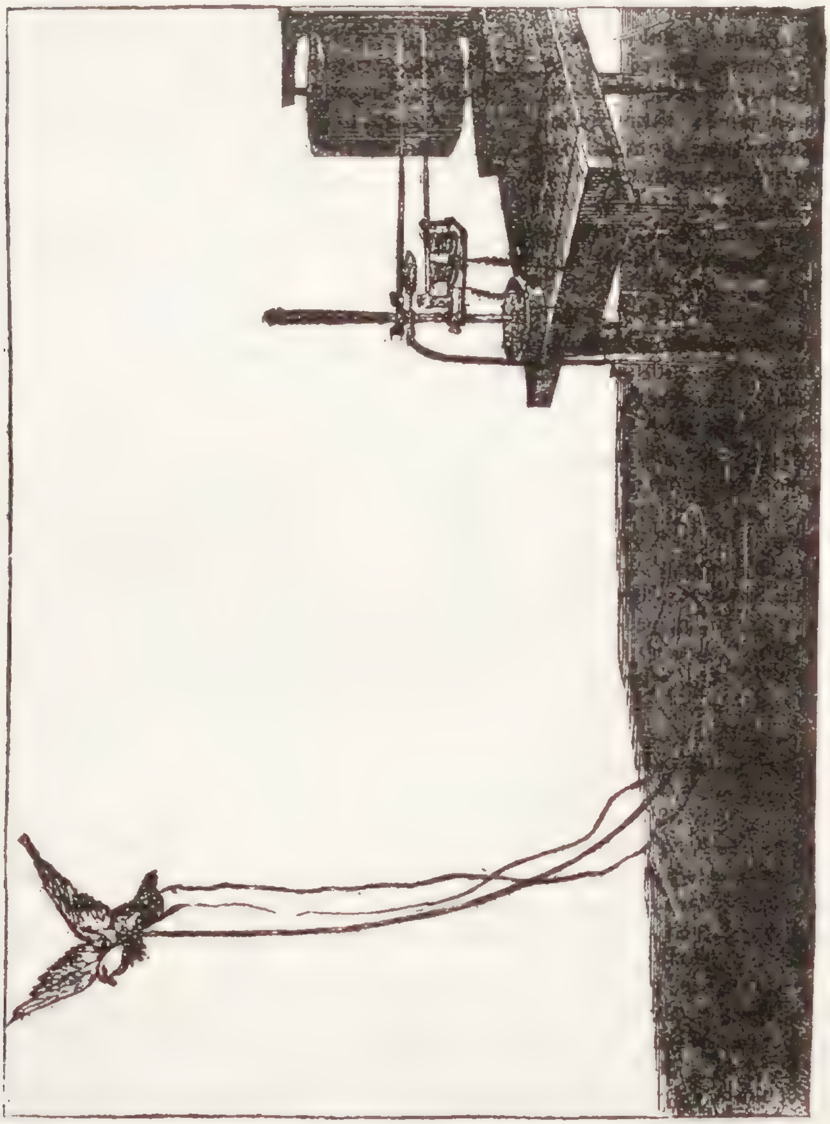
Each animal capable of sustaining itself in the air must develope a force proportional to its own weight, and should possess an amount of muscle proportioned to this weight; for, as we have seen, if the chemical action which takes place in the wings of birds be always of the same nature, this chemical action and the power which it generates will be proportionate to the size of the muscular masses. Now, how is it that the wings of birds in which the surface varies as the square of the linear dimensions suffice to move bodies of which the variation is in proportion to the cubes of these dimensions? Here it is necessary to bring in the theory of power; that is to say, of resistance multiplied by the square of the distance through which it acts in a given time, admitting a uniform rate for the downward stroke of the extremity of the wing in two birds to be compared, and which have the proportion of 1 : 2 in their linear dimensions. The surface of the wings of the larger bird will be, as we have already said, four times as great as that of the smaller one; now, as the resistance of the air against surfaces moving at the same rate is proportionate to their extent, if we call the resistance experienced by the wing of the small bird r, that for the large bird will be $4r$. But these birds, in the downward stroke of their wings do not execute motions of equal amplitude. In the large bird each point of the wing will travel twice as far as the similar part of the smaller bird. If we call the space traversed g, the resistance r, which the wing of the small bird encounters, we shall have rg for the work done by the wing, and $4r$ $2g$ or $8rg$ for the work done by the bird. We see, then, that this work increases in the same proportion as the weight of the animals we are comparing.

Another conclusion results from the preceding considerations. If we admit that the wing possesses the same velocity in both birds, the duration of the stroke will increase with the space traversed by the wing; that is, it will be proportioned to the linear dimensions of the bird. Observation confirms this view by showing that large birds make fewer strokes than small ones do. We have not yet been able to determine exactly the number of strokes of the wings of birds to ascertain if their frequency presents an exact inverse ratio to the size of the animal, but it is easy to see that it is in this manner that the frequency of the wing-strokes of birds varies.

The graphic method, which is easily employed in determining the frequency of the wing-strokes of insects, cannot be similarly employed with birds. It is necessary to adopt some method of transmitting signals from the flying bird to the registering apparatus. For this purpose I have first used the *electric telegraph*, which furnishes the means of solving the following questions:—1. What is the frequency of the strokes of the wings of a bird? 2. What are the relative durations of the periods of elevation and depression of the wings? The experiment consists in placing at the extremity of the wing an apparatus which breaks or closes

an electric circuit at each of the alternate motions, while at the further part of the circuit is placed an electro-magnetic apparatus, which makes a trace upon a turning cylinder. Fig. 16 shows this method of studying

Fig. 16.



Apparatus for registering the motion of the wing of a pigeon by double signals. In one case a small India-rubber tube transmits the record of the muscular action; in the other the periods of elevation and depression of the wing, with their relative durations, are noted by an electric signal.

the flight of a pigeon, together with another method of transmitting signals. In this figure the two wires are separated from each other.

The writing style traces a crenulated line, of which the changes of direction correspond to a change in the direction of the motion of the wing.

In order that the flight may be as free as possible, a fine, flexible cord, containing two wires, establishes the communication between the bird and the writing telegraph. The two ends of the two wires are attached to a very small light apparatus which, from the resistance of the air, executes a kind of valvular motion. When the wing is elevated the valve opens, the circuit is broken, and the line traced by the telegraph rises. When the wing descends the valve closes, the circuit is also closed, and the line is depressed.

Applied to different kinds of birds, this apparatus registers the frequency of the strokes of the wing in each. The number of species which I have as yet been able to study is very small; I have, however, obtained the following results:—

Number of Vibrations of the Wing per second.

Sparrow	13
Wild duck	9
Pigeon	8
Hen-hawk, *Buteo vulgaris*, a hawk called in England and France the "buzzard" or "*busard*"	5¾
Screech-owl	5
Harrier, *Circus rufus*, marsh harrier of England, *buse* of France..	3

The frequency of the strokes varies according as the bird is starting, is in full motion, or at the end of its flight. Some birds, as we know, have periods when the wing is motionless, and when they move by means of the momentum acquired.

It is interesting to observe the relative duration of the periods of ascent and descent of the wings. Contrary to the opinion expressed by some observers, the descending period is generally longer than that of elevation. The inequality of the two periods is especially evident in birds which have large wings and make few strokes. Thus, while the periods are almost equal in the duck, which has very narrow wings, they are unequal in the pigeon, and much more so in the harrier.

The following figures exhibit the results obtained from several species of birds:—

Species.	Total distance traversed during one complete oscillation of the wing.	Proportional distance.	
		Ascent.	Descent.
Duck	6·66 centimetres per second	3·0	3·66
Pigeon.........	7·5 centimetres per second	3·0	4·5
Harrier	21·5 centimetres per second...........	8·5	13·0

It is more difficult than might be supposed to determine the precise instant of the change of direction in the line traced by the telegraph. The attraction of the magnet and the relaxation have an appreciable duration, if the blackened cylinder turns with sufficient velocity to measure the rapid motions which we seek to analyze. The inflections of the line traced by the telegraph then become curves, of which it is somewhat difficult to determine the precise origin. There is therefore a limit to the precision of the measurements which can be made by the electric method. I think that we cannot approximate by this method nearer than $\frac{1}{100}$ of a second to the duration of a motion.

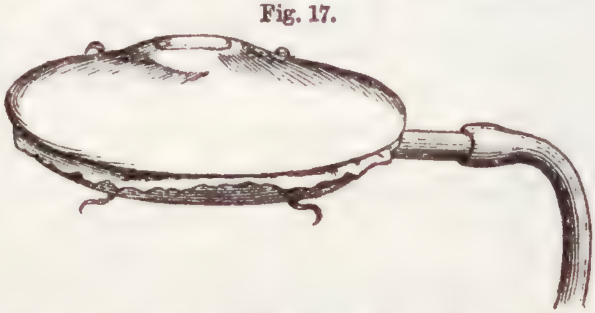
Another kind of signal allows the estimation of the frequency of the stroke at the same time that it furnishes indications of the successive action of the principal motive muscles of the wing.

Myographic method.—In 1867 I indicated a myographic method which might be applied without mutilating the animal upon which the experiment was performed. It consists in employing the swelling of a muscle to afford evidence of its changes in length—that is to say, by its contraction or relaxation. Muscles, not being sensibly compressible, cannot change their length without at the same time changing their transverse diameter. A rapid or short, feeble or energetic contraction of a muscle, hence, is accompanied by an increase in diameter, affording the same features of rate or intensity. At each descent of the bird's wing the great pectoral muscle thus exhibits an increase of size, which can be indicated by the registering apparatus.

I have made use of flexible air tubes of India-rubber in transmitting these effects, a method which has enabled me at times to register at some distance the beating of the heart, the pulse, and the motions of respiration.

The bird flies in an enclosure fifteen metres square and eight metres high. The registering apparatus being placed in the centre of this enclosure, twelve metres of rubber tubing are enough to establish a constant communication between it and the bird. A sort of corset is applied to a pigeon (*see* Fig. 16). Under this corset, between it and the pectoral muscle, is placed a little contrivance intended to exhibit the swelling of the muscle. It consists of a small shallow metal basin containing a spiral spring, and closed over by a thin sheet of rubber. This basin, thus closed, communicates with the transmitting tube.

Fig. 17.



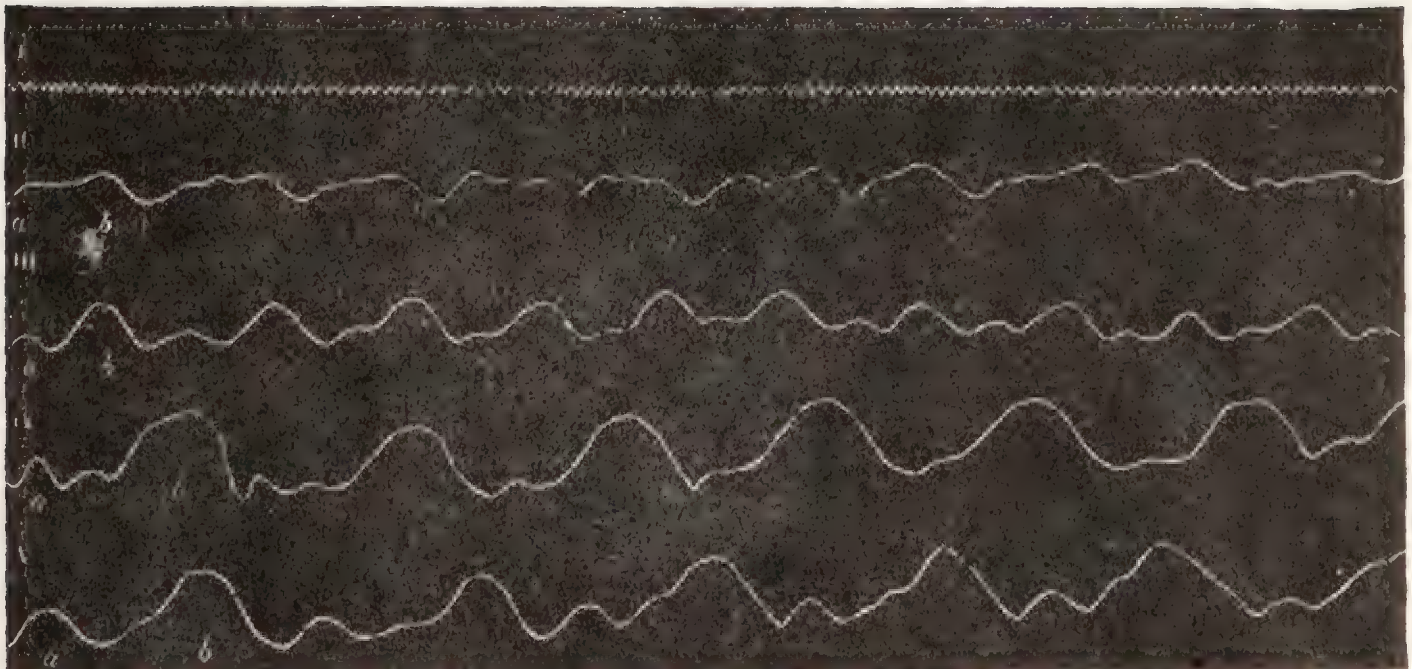
Apparatus for exhibiting the contraction of the thoracic muscles of birds. The upper convex face is formed of a sheet of rubber, held up by a spiral spring, and is applied to the muscles. The lower face, in contact with the corset, carries four little hooks which are caught in the cloth and hold the apparatus in its place.

Any pressure applied to the face of the apparatus depresses the rubber. The air is forced out of the basin and escapes by the tube. If the pressure ceases, the air re-enters the basin in consequence of the elasticity of the spring which raises the rubber. An alternate inspiration and aspiration is by this means established in the tube, and the motion of the air transmits to the registering apparatus a signal of the more or less intense pressure which has been exerted upon the rubber cover of the basin. The registering apparatus I have used in all my experiments is also composed of a basin, covered by a rubber membrane communicating with the transmitting tube. The motion imparted to the first basin is transmitted by the air to the rubber cover of the second. The motions of the membrane of the receiving apparatus, amplified by a lever, are written on the smoked cylinder. Fig. 16 represents the general arrangement of the experiment in which the electric telegraph and transmission by air are exhibited together. We see the pigeon under experiment furnished with its corset and apparatus for showing the movements of its pectoral muscles. The transmitting air-tube ends at the registering apparatus, which writes on a revolving cylinder. At the extremity of the pigeon's wing is an arrangement which opens or closes an electric circuit as the wing rises or falls. The two wires of the circuit are represented separately, and two cells of Bunsen's battery are seen in their connection with the helix, which, furnished with a lever, registers the telegraphic signals of the motions of the wings. One precaution is indispensable—the rubber tube which connects the bird and the apparatus must be prevented from stretching. When the bird flies it raises more or less of the tube, and if this is elastic it will become elongated by its own weight, producing a rarefaction of the air contained in the two receptacles, and the registering lever will trace muscular curves on a descending line. To prevent this inconvenience, the tube may be tied here and there to the telegraphic cord by means of ligatures, taking care that the tube is a little longer than the cord, and that it is not subjected to traction. These precautions being taken, nothing prevents the successful transmission of signals. No trouble need be taken in regard to the elasticity of the tube in a transverse direction; its walls are so thick that their elasticity is not brought into play by the feeble changes of pressure to which the air they contain is subjected.

The bird is let loose at one end of the enclosure, the dove-cote in which it is ordinarily kept being placed at the opposite end. The bird naturally flies toward the latter. During its flight the tracings represented by Fig. 18 are obtained.

The trace is seen to differ according to the kind of bird experimented upon. However, in all the traces we perceive the periodical return of two motions, *a* and *b*, which are produced in each vibration of the wing. What is the signification of these two muscular actions? It is readily seen that the undulation *a* corresponds to the action of the muscle which elevates the wing, and *b* to that of the muscle which depresses it. This can readily be proved by comparing the trace of the muscular action in the electric trace of the elevation and depression of the wing. These two tracings, placed one under the other, show that the period of elevation of the wing agrees with the extent of the undulation *a*, and the period of depression with the undulation *b*.

Fig. 18.



Myographic tracings of the pectorals obtained from various kinds of birds during flight. I. Tracing of the tuning-fork to be used in measuring the absolute duration of each muscular motion; this tuning-fork vibrates 200 times a second. II. Tracing of the muscles of a pigeon obtained, as in Fig. 16. III. Tracing of a wild duck. IV. Tracing of hen-hawk V. Tracing of a harrier.

But to establish this agreement we must take the unequal rapidity of the transmission of the electric and aërial signals into account. We may consider the electric transmission as instantaneous, while the aërial transmission is at the same rate as the rapidity of sound through the air, that is, 334 metres per second. If the points of the two styles are placed vertically one above another, the tracings will not be exactly superposed, but the electric signal will precede the other by a distance corresponding to a certain fraction of a second, according to the length of the tube which has been employed. We can even compute, from the length of the air-tube, the amount of retardation, but it is more certainly ascertained by a special determination for the particular tube which may be in use. In a previous experiment, motions were simultaneously transmitted by the tube and by electricity, and the discrepancy determined. In the apparatus which I am using, the constant discrepancy is ·04 of a second. I should therefore set back the electric signals by a corresponding distance, in order that they may agree with the signals transmitted by the air-tube. Fig. 19 shows the superposed tracing from a harrier after correction.

It is easy to understand how the undulations *a* and *b* are produced in all the tracings of the muscles of birds. In fact there exist two distinct planes of muscles in the upper part of the region investigated near the end of the sternum. The most superficial is formed by the great pectoral which lowers the wing, the deeper by the median pectoral or elevator of the wing, the tendon of which passes behind the bifurcation of the sternum to attach itself to the head of the humerus. The two superposed muscles act by their swelling upon the apparatus applied to them. The median pectoral swells when it contracts, signalizing the undulation *a* by its action; the great pectoral signalizes the lowering of the wing in the undulation *b* in a similar manner.

We can verify the correctness of this explanation by a very simple experiment. Anatomy shows us that the median pectoral is narrow, and only covers the inner portion of the great pectoral along the keel of the sternum. So if we displace the little apparatus which reveals the motion of these muscles, and carry it further outward, it will occupy a region where the median pectoral does not cover the great pectoral, and the tracing only presents a simple undulation which corresponds to *b* in the figures.

It is, therefore, sufficiently demonstated that the undulations *a* and *b*, in the muscular tracings of the birds upon which I have experimented, correspond exactly to the principal elevating and depressing muscles of the wing; but we cannot attach much importance to the form of these tracings for deducing the precise nature of the motion effected by the muscle. In fact, these motions appear to override one another. So the relaxation of the median pectoral is probably incomplete when the great pectoral commences to act. We should expect no more from these tracings than they naturally furnish, that is to say, the number of vibrations of the wing, the greater or less regularity of its movements, the equality, inequality, and energy of each of them. Restricting the enquiry within these limits, the experiments show that the strokes of the wings of birds differ in frequency and amplitude in the different moments

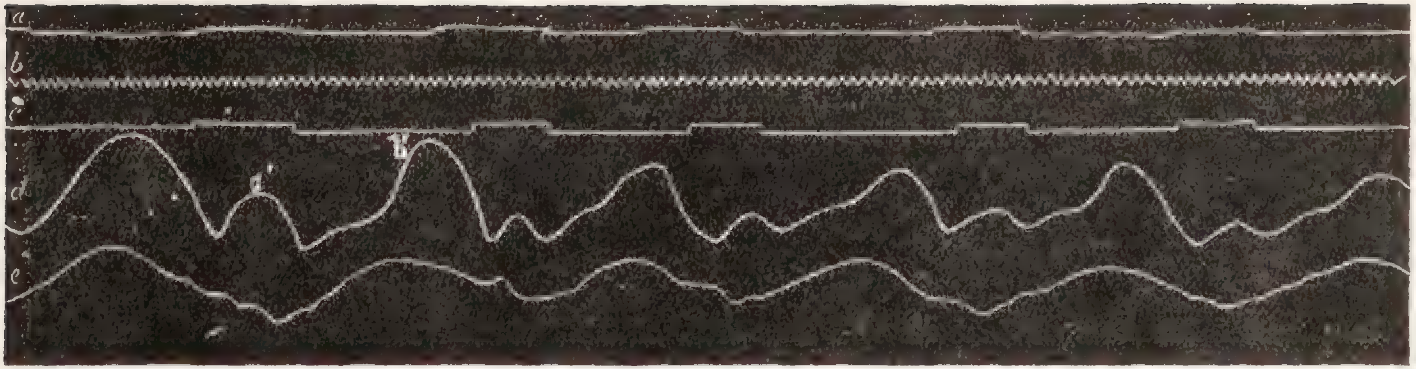


Fig. 19.—Line *a* represents the electric tracing of the ascent and descent of the wing of a harrier, as furnished by the apparatus. Line *b* is a tracing of a tuning-fork vibrating 200 times a second. Line *c*, correction of the electric tracing, which latter does not represent the changes with sufficient abruptness in the figure (*a*) obtained directly from the wing. Line *d*, tracing of the action of the pectoral muscles in the harrier by the air apparatus; *a′*, period of elevation of the wing; b′, period of depression. Line *e* will be hereafter referred to; it represents the vertical oscillations of the bird during flight.

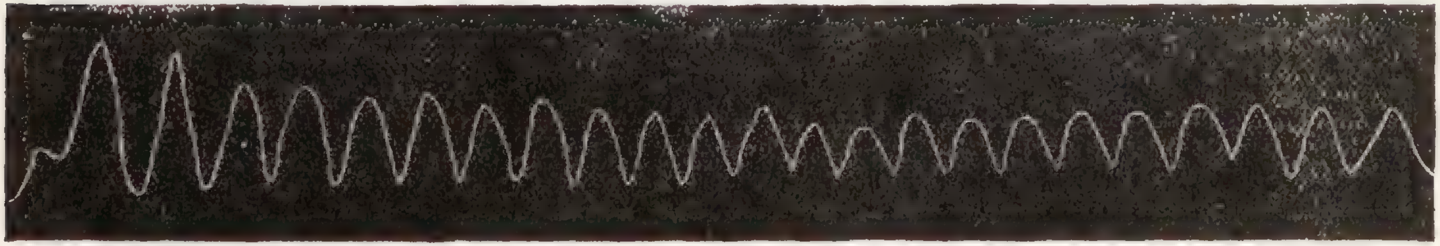


Fig. 20.—Showing the difference in amplitude and frequency in the wing-strokes of a pigeon during a flight of fifteen metres. To the left the extended traces indicate the movements at the commencement of flight. This tracing was recorded on a cylinder which moved very slowly, allowing the record of a large number of strokes to be compressed into a small space.

of flight. At starting the strokes are fewer but more energetic; they attain, after the first two or three, a regular rhythm, which they lose at the moment when the animal is about to alight.

We shall find in other experiments more complete indications of the variation of the movements of the wing during the different periods of flight.

Such are the certain indications which can be derived from the method of signalizing established between the flying bird and the registering apparatus. But if it is wise to guard our conclusions by more rigorous experiments, it may at least be permitted us to attempt to discover whether the tracings of these muscles cannot furnish us with further information in regard to the motions from which they are derived. I have elsewhere demonstrated that the form of the motion produced by a muscle when it is excited varies according to the resistance which this motion encounters. Thus, in applying the myograph to the muscle of a frog, I have seen that if contraction be impeded by an obstacle the duration of the muscular shock becomes greater on account of that obstacle. Theory, also, would foretell us, that if the muscle presents certain modifications in the different phases of its contraction, the result of unequal resistance overcome at different periods, the swelling of the muscle should also present the same phases. If the tracing is the exact impression of the motions produced by the muscle, it can inform us of the nature of the resistance which the wing of the bird encounters in the different phases of one of its vibrations.

Let us take the most simple example. As the median pectoral and great pectoral are very unequal in size, we may suppose that if the resistance is equal in the two periods of elevation and depression, the duration of the former would much exceed that of the latter; and, as exactly the contrary is the case, we may conclude that the rising wing does not strike the air but cuts it apparently with its edge, so that the resistance to the elevation is very feeble, and is very strong to the depression of the wing. Now, if we examine the tracing of the depression of the wing we shall find there, within certain limits, the expression of the different amount of resistance which the wing encounters in the different phases of its depression. It is necessary by previous experiments to determine the effect of certain special kinds of resistance, which we may call elastic resistance, in order to better understand the signification of different forms of muscular motion.

Let us take the muscle of a frog, apply it to the myograph, and excite contraction in it by means of electricity. The form of this contraction varies in the following manner under the influence of different kinds of resistance opposed to the action of the muscle: If a weight be suspended to the muscle it gives the tracing *a*, Fig. 21. If it encounter an absolute obstacle to all further diminution of length, after a few instants of contraction it gives the trace *b*. Finally, if it encounters an elastic obstacle, as a rubber thread, which presents a surmountable resistance, the muscle gives the curve *c*. It seems as if these different forms were sufficient to characterize the nature of the resistance that the contraction of the muscle has had to overcome.

In the first case it is the inertia of a body; now this body submitted

to the muscular force during a limited period, should have an accelerated motion at first and then a diminishing motion. This is precisely what the form of the curve *a* indicates. In the second case it is not necessary to explain how the horizontal line which forms the summit of the curve *b*, expresses the cessation of all contraction in the presence of an absolute obstacle. Lastly, in the curve *c*, the presence of an obstacle is betrayed by a deflection of the curve; that is, by a change in the rapidity of the motion which produces it; but the contraction does not cease because the obstacle is not insurmountable, but it becomes slower on account of the greater resistance presented.

I have been able to convince myself that in the above-mentioned experiments the swelling of the muscle presents the same phases as its change of length. In fact, I have transmitted to the myograph the motion produced by the swelling of the muscle, and have obtained tracings identical with the preceding. Finally, wishing to know if the apparatus which I have used would faithfully transmit the different phases of the swelling of muscle, I made the following experiment: I applied the little drum which had served to obtain the tracings from the birds (Fig. 18) to my own biceps muscle, fixing it exactly in place by means of a bandage, and put it in communication with the registering apparatus. I then made sudden voluntary motions, as similar as I could make them to each other, but applied to overcome various forms of resistance. In one case I lifted a weight; in another my hand was absolutely arrested in upward motion by being placed beneath a heavy table; in still another, I tied my hand to a fixed object with a rubber band which, by a short flexure of my fore-arm, required the utmost efforts of the muscle to stretch it.

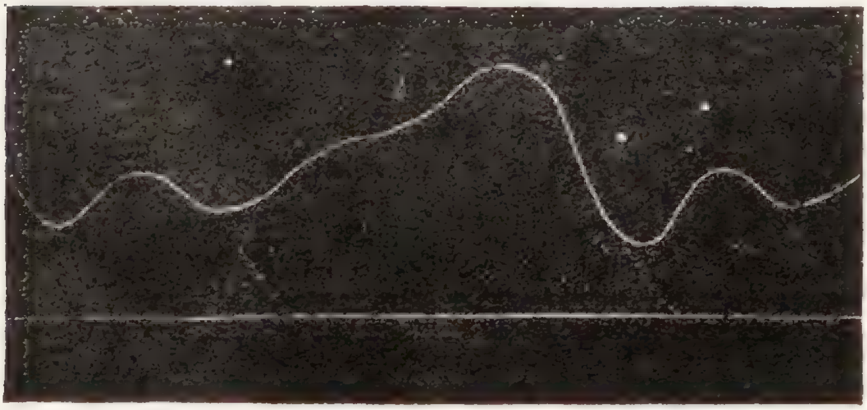
Now the tracings which express the swelling of the biceps in these three experiments reproduce the three types represented in Fig. 21, and

Fig. 21.

show very clearly that voluntary exertions had been subjected to different forms of resistance. I tried to force upon the muscles identical motions in each case, which was always a short vigorous flexure, but the nature of the resistance modified these muscular actions which were intended to be similar to each other, and imparted to them the various

phases and durations which are exhibited in the figure. This being settled, let us return to the muscular tracing of the great pectoral of the bird. I have said that the exact commencement of this motion is undetermined, the elevator of the wing not having fallen into repose before the depressor commences to act, and if we would represent the probable curve of the action of these two muscles from that which the myograph obtains for us, it will be necessary for us to complete the tracing by means of dotted lines as in Fig. 22.

Fig. 22.



Trace of the action of a harrier during flight: *a*, action of the elevating muscle; *b*, of the depressing muscle. The dotted lines which descend to the axis of the curve complete the probable form of the motions of the two muscles of the wing.

Thus reconstructed, the form of the curves of the elevator and depressor reveals the nature of the resistance which each of these muscles has encountered. The curve *a* of the median pectoral is that of a muscle acting on a weight; it seems to indicate that the inertia of the wing is the only obstacle which the elevator muscle has to overcome. The curve *b* shows us a deflection, during part of which the contraction of the muscle takes a slower motion; it is here that the resistance of the air is interposed. These things happen, then, exactly as in the experiments which I have made upon my own muscles and those of the frog. But you may ask why the deflection of the curve is not produced sooner; and if the depressor muscle can rapidly contract for a certain period before encountering sufficient resistance from the air to impede its motion. This is just what happens; we have the proof of it in the anatomical disposition of the attachments of the great pectoral muscle. We shall see hereafter how the motion of the humerus around its articulation is produced; at present I will only say that in the first part of its action the great pectoral in contracting produces a pivot-like motion of the wing upon the head of the humerus, and that in this first motion the muscle does not experience the resistance of the air which retards its contraction an instant later.

The reader will perhaps consider that an inordinate number of deductions are made from the forms of the curves of the muscles; but those who will familiarize themselves with the use of the registering apparatus, and in particular with the myograph, will soon be convinced that chance does not enter into the formation of the curves, but that the details should find their explanation in the dynamic conditions of the production of muscular power.

Motions executed by the wing of a bird during flight.—We have seen, in regard to the mechanism of the flight of insects, that the fundamental experiment has been that which has shown the trajectory of the point of the wing in each of its evolutions. The knowledge of the mechanism of flight flows, so to speak, naturally from this first idea. The same determination is equally indispensable for the flight of birds, but the optic method is here inapplicable; the motion of a bird's wing, while too rapid to be followed by the eye, is not sufficiently rapid to form a persistent impression of its entire trajectory upon the retina. The graphic method, which I have hitherto employed, only furnishes impressions of motions which happen to follow a straight line, and it is only by combining this rectilinear movement with the revolving cylinder with a smoked surface that the expression of the rapidity with which the motion is effected at each instant is obtained.

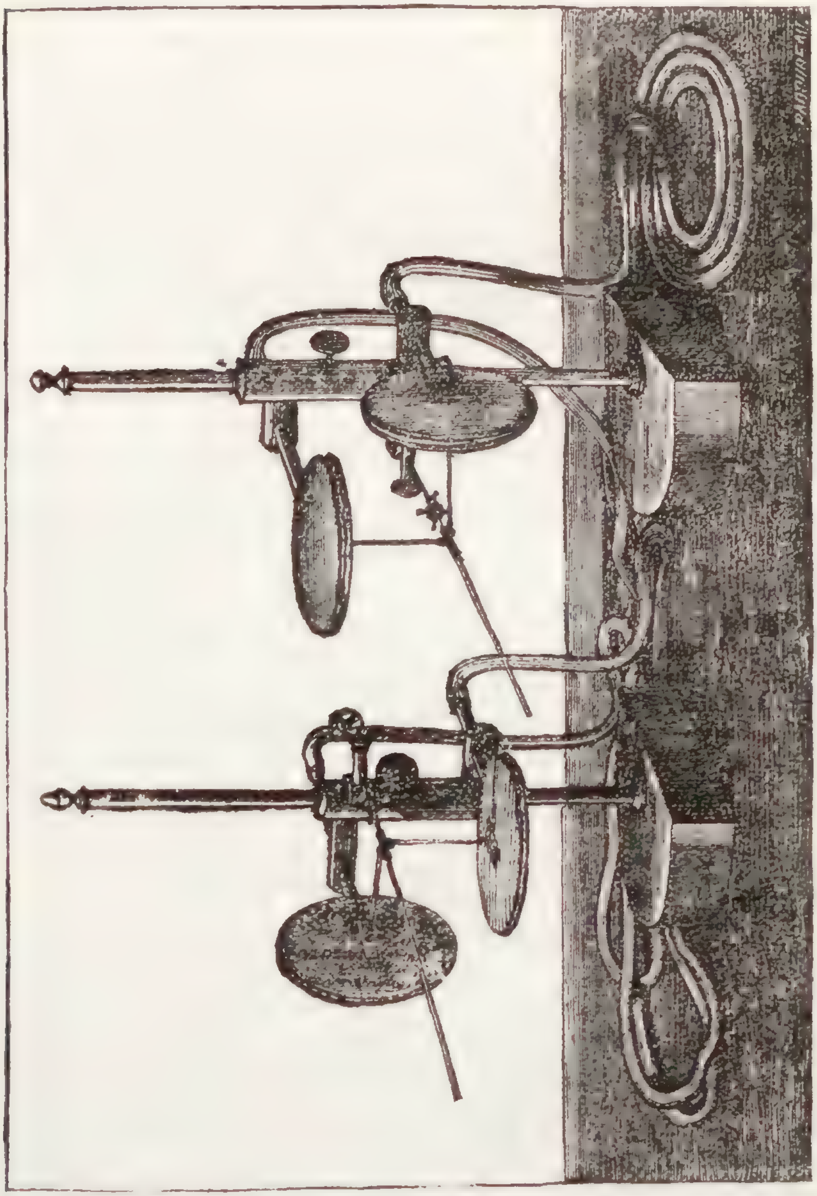
The problem is to find the means of registering on an immovable plane all the motions which the point of a bird's wing makes in space, as if a style had been placed at the end of the wing, and this style traced or rubbed on a piece of paper by its side. It is still further necessary to have a figure of the same nature as the luminous figure of the gilded wing of an insect, that the piece of paper on which the trace is to be made shall remain motionless in regard to the centre of motion of the wing of the flying bird, or in effect that it shall follow the bird in all its phases of impulsion through space.

Now, physics teach us that all motion susceptible of registration in one plane can be generated by the rectangular combination of two rectilinear motions. The tracings obtained by Kœnig by arming a vibrating Wheatstone's rod with a style, the luminous figures of musical chords which M. Lissajous has produced by the reflection of a ray of light from two vibrating mirrors perpendicular to one another, are well known examples of the formation of a plane figure by means of two rectilinear movments. Thus, admitting that the motions of elevation and depression of the wing can be transmitted at one time, as well as the back and forward motions of this organ, by supposing that a writing style can simultaneously receive the impulse of these two motions, perpendicular to each other, this point will write on the cylinder the exact figure of the motions of the bird's wing. I tried at first to construct an apparatus which would thus transmit such a motion to a distance and register it, without concerning myself with the way in which I might apply this rather weighty mechanism to the bird.

Fig. 23 represents this provisional apparatus, the description of which is indispensable for the comprehension of the second mechanism, which I shall describe hereafter. Upon two solid feet, carrying vertical supports, are seen two horizontal arms parallel to each other. These are

two aluminium levers which, by the transmitting apparatus to be described, should both execute the same motions. Each of these levers is mounted on a ball-and-socket joint, or double articulation, which

Fig. 23.



Apparatus intended to transmit to a lever at a distance all the motions executed by another lever around one of its extremities.

permits all kinds of motion; thus each lever can be carried above, below, to the right, or to the left. It can by its point describe the base of a cone of which the joint will be the apex. In fact, it will execute any kind of motion which the experimentor may choose to impart to it. It is also necessary to establish the transmission of motion from one lever to the other at a distance of ten or fifteen metres. This is done by means of a process with which the reader is already familiar—the use of drums and air tubes.

The lever, which is seen at the left in the figure, is fastened by a metallic arm articulated at one of its extremities to the membrane of a drum placed below it. In the vertical motions of the lever the membrane of the drum rises or falls by turns, producing a throbbing motion of the air in another drum through a long tube, which establishes a communication between them. In the apparatus to the right in the figure, the second drum is placed above the corresponding lever articulated with it, and faithfully transmits all the motions which have been imparted to the first drum to the left. These movements will be in the same direction in both levers on account of the inversion of the position of the drums. If we depress the first lever it presses down the membrane of the drum below it, inducing a pressure which lifts the membrane of the second drum and consequently lowers the second lever; conversely the elevation of the first lever produces an influx of air, which raises the membrane of the second lever.

Proceeding in the same manner to transmit motions in a horizontal plane, I have placed at the right of one of the levers and at the left of the other a drum with the membrane in a vertical plane, which imparts lateral motions to these levers; these motions are transmitted by a special air-tube, as before. In the apparatus thus constructed, if we move the end of one of the levers with the finger, the other lever will be seen to execute the same movements with perfect fidelity. The only difference consists in a slight diminution of amplitude. This happens because the air contained in the tubes and drums is slightly compressed, and in consequence does not transmit the whole of the motion which it receives. It is easy to remedy this defect, if it be one, by placing the ball-and-socket joint a little nearer the point whence the motion is transmitted to the second lever. But it is better not to attempt too great amplification, because the friction is thus augmented and the force which should overcome it is diminished.

After having determined that the transmission of such motion can be effected in a satisfactory manner by means of this apparatus, I have sought for the means of tracing these movements upon a plain surface. The difficulty which before presented itself when I endeavoured to apply the graphic method to the study of the wing-strokes of insects, again appeared, but this time there was no means of eluding it, and I contented myself with partial tracings. The point of the second lever described a spherical figure in space which could not be tangent, except as a point, to the smoked surface, which should receive the trace. In consequence, I should have to register the projection of this figure on the plane. Helmholtz has also encountered the same difficulty in the construction of his myograph, and had solved it by causing the point of the writing

style to rub continually on the smoked surface by means of a weight. But as I could not attach a weight to the extremity of my lever, I resorted to the following expedient, shown at the end of the lever in Fig. 24. It is large at the base in order to resist all lateral deviations

Fig. 24.

Elastic point tracing upon smoked glass.

from friction ; this base is fixed on a vertical piece of aluminium which is attached to the extremity of the lever. In this way the point of the contrivance, which performs the office of a style, is situated exactly opposite the end of the lever whose motions it registers. If the lever be elevated and takes the position indicated by the dotted lines in Fig. 24, in traversing this space it has described the arc of a circle, and its extremity will be no longer on the same plane as before, but the elasticity of the contrivance will have carried the point of the style forward, and it will therefore continue to be in contact with the plane upon which it is tracing. Thus the lever elongates or shortens according as the case requires, and its point continually rubs upon the plane. I should add that the surface upon which the tracings are received is of finely polished glass, and that the contrivance which I have used is so delicate that the pressure which it exercises produces scarcely any friction.

The apparatus being thus constructed, it must be submitted to verification, to ascertain whether the motions are faithfully transmitted and registered. To do this both levers of Fig. 23 are furnished with similar styles placed against the same smoked glass ; and moving one of the levers with the hand, for instance, so as to write my name, the other lever should reproduce the same signature. It frequently happens that the transmission is not equally good in both directions, which is perceptible by the deformity of the transmitted figure, which is increased more or less in height or breadth. This deficiency can always be corrected, since it is due to the membrane of one of the drums being stretched more than that of the other, and hence yielding less easily to pressure. It is very easy to equalize the tension by tightening the membrane of the other drum until the figure traced by the first lever is identical with that traced by the second.

The modifications by means of which I have rendered this transmission applicable to the study of the motions of the wing of a flying bird, are as follows :—

The apparatus necessarily being heavy, it required a large bird to carry it. Strong adult harriers served for the experiments. I fixed a light strip of wood upon the bird's back, upon which the apparatus was placed, by means of a kind of corset, which left the wings and feet free. That the lever might faithfully execute the same motions as the bird's wing, the joint of the lever should be placed in contact with the humeral articulation of the harrier. As the presence of the drums by the side of the lever does not permit this immediate contact, I had recourse to a parallelogram, which transmitted to the lever of the apparatus the movements of a long arm of which the centre of motion was very close to the articulation of the bird's wing. Finally, to obtain an identity of motion between the arm and the harrier's wing, I fixed on the bastard wing, that is to say, on the metacarpal portion of that organ, a well cut screw-vice, furnished with a ring, through which passed the steel arm of which I have just spoken.

Fig. 25 represents the harrier flying with the apparatus in question ; below hang the transmitting tubes of the registering apparatus.

Fig. 25.

Harrier flying with the Apparatus, which transmits the motions described by the extremity of its wing.

After a great many fruitless attempts and changes of construction of the apparatus, which, being very fragile, broke at almost every flight of the bird, I succeeded in obtaining satisfactory results. During flight the registering lever described a kind of ellipse, but I was obliged to give up registering this figure upon a stationary glass. The motions of

the wing differing at different moments of flight, the style did not pass over the same points, and I obtained a very confused tracing. I then resolved to use a glass moving horizontally at a uniform rate in order to obtain an extended figure, which I could afterward submit to a geometric correction, and thus obtained as it would be if traced on a stationary surface a figure for each instant of flight.

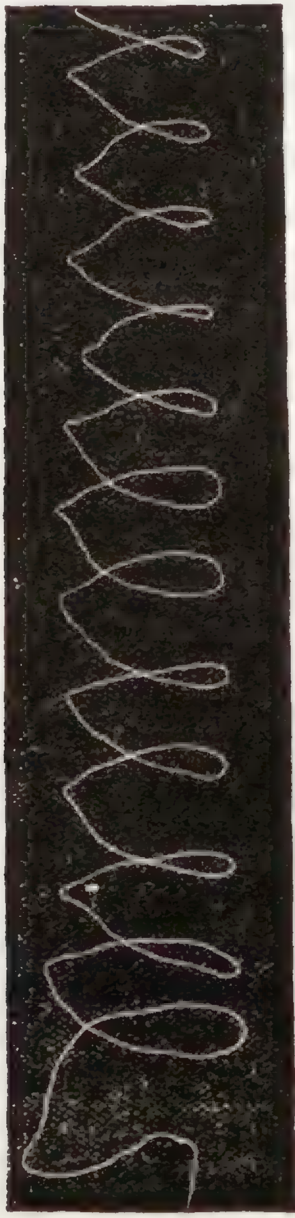


Fig. 26.

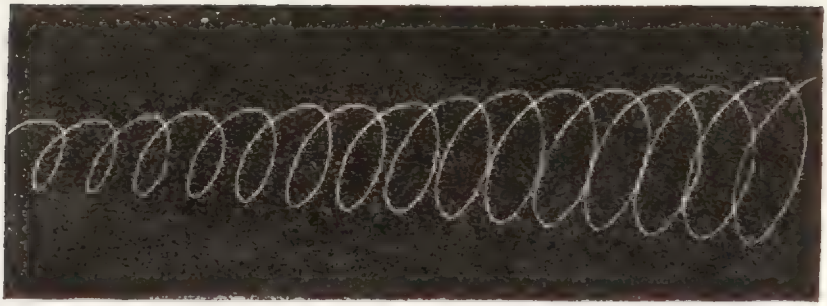
Representing the course of the point of the wing at each moment of flight.

Fig. 26 represents one of the numerous tracings which I have thus obtained. The perfect uniformity of these tracings gives me entire confidence in their correctness. To analyze the meaning of this curve it is necessary to know how the bird flies, how the apparatus is arranged, and in what direction the smoked glass moves while receiving the tracing. The observer being placed opposite the glass on the smoked side, sees it move from the right to the left; between the glass and himself is a tracing apparatus with the lever rubbing upon the smoked surface directly in front of him. The bird flying from right to left, in a plane parallel with that of the glass, carries the lever of the apparatus on his right wing, so that the respective levers of the two machines are always parallel to each other. This being known, the tracing should be read from left to right. We have seen that the tracing consists of a kind of ellipse, which the motion of the glass extends into a spiral. The movements, more extended at the beginning of flight, gradually lose a little of their amplitude, and retain a uniform character for some time.

This figure somewhat resembles that which we obtain from a Wheatstone's rod, according to the unison which traces the ellipse which its point describes upon a surface moving from right to left. Fig. 27, showing the tracing of this rod, admits the comparison of the two.

The wing of a harrier thus describes a sort of ellipse, but it is necessary to determine more exactly its shape, and to correct the error caused by the motion of the glass plate.

Fig. 27.



Ellipse traced by a Wheatstone's rod upon a turning cylinder.

Such a correction is impossible unless we know the elevation attained by the wing at the end of successive and equal intervals of time. This once obtained, if we trace parallel horizontal lines representing the position of the wing at each of these successive moments, these lines will cut the descending curve at points which correspond to the successive equal intervals of its course. It is clear that if these successive points of the curve have been produced at equal intervals of time, each of them, under the influence of the motion of the glass plate, will have a constant deviation toward the right, bearing a stated relation to the preceding point. The correction thus consists in carrying the second point back toward the left twice this amount, the third point three times this amount, and so on. The ascending portion of the curve should also be submitted to this correction, and similarly each part of the tracing. But it is precisely the height which the wing attains in the different ascending and descending motions of its course which we do not know; but this want can be supplied by the apparatus in the following manner:—

Since the principle of this mechanism is founded upon the transmission of two motions, perpendicular to each other, vertical and horizontal, it suffices to suppress the transmission of the horizontal motion to obtain the curve of elevation immediately; that is to say, the expression of the height of the wing at each instant of its course. For this I obstruct the tube of lateral transmission, let the bird fly, and obtain the curve of the heights of the wing at each moment.

The correction being made, and Fig. 26 being selected to show the course of the point of the wing during one of its evolutions, and projected upon a stationary plane, we obtain Fig. 28.

Fig. 28.

Course in space of the extremity of the wing, reduced from the motion of the bird.

The arrows indicate the direction in which the wing moves.

Is this the form characteristic of all birds; or is it only that of the harrier in the conditions of flight in which it has been placed?

The last supposition appears to be the most probable; we can see, even while comparing the form of the tracing at different instants of its flight while under experiment, that the ellipse is greater and more open in the first strokes of the wing than in the last. It is, however, necessary to except the second stroke of the wing, which has given me a narrower ellipse than in any other in all the experiments which I have made. I do not know to what this special form is to be attributed, but have thought it worth while to mention it on account of its constancy.

Of the rotation of the humerus and the changes of the plane in the wing during flight.—The wing of a bird, like that of an insect, must meet with a sufficient resistance from the air in its motion upward and downward to incline its flexible portion, namely, that which forms the webs and coverts. This cause does produce a change of the plane of the wing, but there is another even more powerful, for it places the wing at the outset of the depressing motion in a favourable position for the double propulsion which is produced. I refer to the pivot motion which the humerus executes around its axis at each contraction of the great pectoral. It is enough to examine the bony crest on which the large tendon of the great pectoral is inserted, and to consider that this crest is situated on the anterior edge of the humerus, to comprehend that the action of the great pectoral, whose fibres are carried backward and downward, should produce a rotary motion of the humerus around its longitudinal axis. The conformation of the humeral articulation is perfectly adapted to this motion. Finally, the existence of this rotation is rendered still more necessary by the resistance which the air presents to the back of the wing and opposes to the descent of its feathered portion. We can demonstrate the existence of this motion and measure its extent by means of the registering apparatus. But I have thought it best to defer these researches, especially as they necessitate the construction of special apparatus, which would require numerous experiments, and would produce, after all, results of very slight importance. In fact, we are enabled to deduce from the attachment of the muscles the nature of the motion which they produce, and this deduction is especially easy.

I have always sought to verify the existence of this rotary motion of the humerus, and to measure its extent, by the application of electricity to the muscles of the bird. In the experiment for measuring the static power developed by the contraction of the great pectoral muscle, previously described, I noticed that at each excitement of this muscle the humerus executed a rotary motion upon its axis. I fixed in the humerus a rod, erpendicular to its axis, and was enabled, by the angle formed by the two positions of this rod, to demonstrate that the rotation in the harrier corresponded to an angle of thirty-five or forty degrees. It seemed that the limits of this angle were fixed by the attachments of the median and great pectoral muscles. If traction be exerted upon the two antagonistic muscles of a newly-dissected bird, it will be seen that the median pectoral raises this member so that its upper face is turned somewhat backward. The action of the great pectoral changes this position of the wing completely, and carries its upper face strongly upward and even a little forward. These expressions, upward and

downward, are relative to a plane cutting the bird into a dorsal and a ventral half; but this plane, doubtless, is not entirely parallel with the horizon during flight. But it is certain that the resistance of the air should give a much more pronounced deflection to the feathers during the more rapid descent of the wing.

The most difficult to measure of the influences which change the plane of the bird's wing is that which relates to the pressure of the air on the feathers. Perhaps it may not be impossible to devise an apparatus capable of measuring it, but it so varies with the variations of the velocity with which the wing is lowered, that any measurement which might be obtained would be only the expression of a particular case. It is very probable, on the contrary, that the change of plane due to the action of the pectoral muscles is a much more constant phenomenon. We can infer the action of the two motions of the bird's wing from what has been said of the mechanism of the flight of insects. It is evident that the descent of the wing will have the double effect of raising the bird and of imparting to it a horizontal motion. As to the ascent of the wing its office cannot be the same, because the imbrication of the feathers does not offer a resistant surface to the air.

Everything tends to show that the ascending wing cuts the air with its anterior edge, but, as we shall see, another phenomenon occurs which uplifts the body of the bird during the elevation of the wing; this is the transformation of the impulse which the bird has acquired during the lowering of the wing. This impulse is changed in rising, by a mechanism analogous to that which raises the toy kite.

In a remarkable study of the flight of birds, M. Liais has been led, through observation and deduction, to adopt this theory, to which the experiments about to be described, I trust, will add new proofs in its favour.

Before leaving the subject it is necessary to mention the existence of certain other motions in the flight of small birds. I refer to the folding and unfolding of the wings. But the existence of these motions does not seem to be constant, and the eye cannot perceive the least trace of them during the flight of the large birds upon which I have experimented. I shall, therefore, omit the study of these motions, and of their possible effects, and restrict my conclusions on the mechanism of flight to a certain number of determinate species of birds.

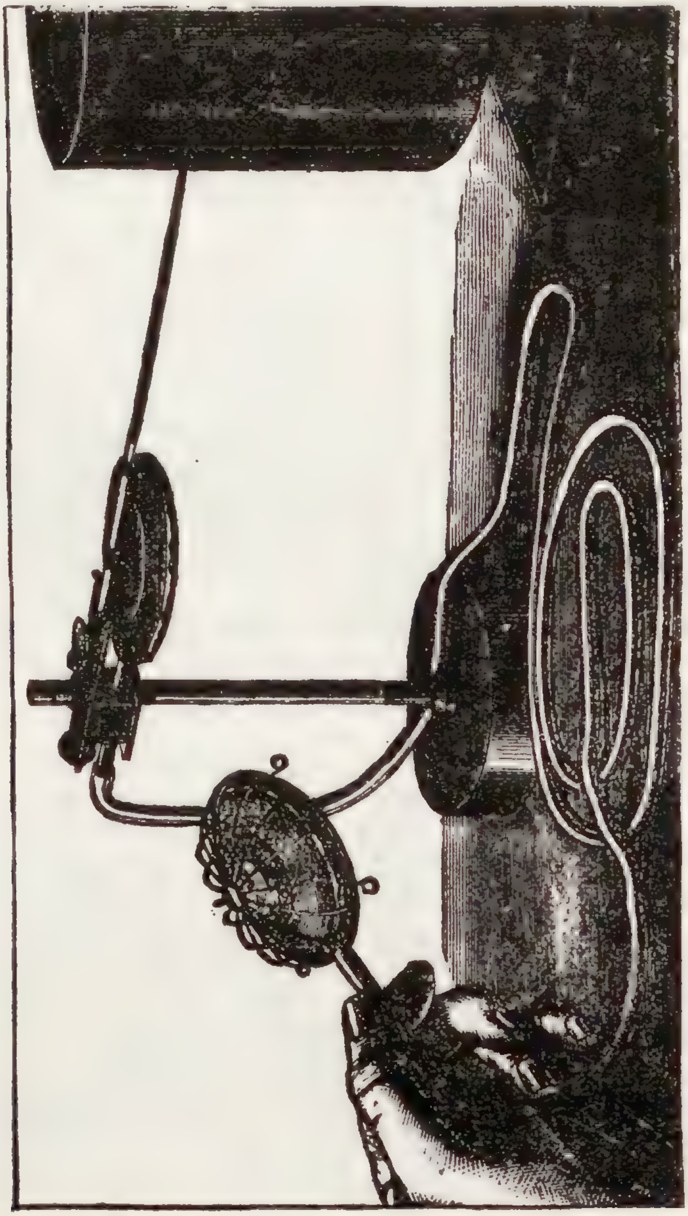
The study of the motions of the wings of birds during flight necessarily includes the effect produced by each of these movements. We are tempted to deduce these effects from the nature of the motions which generate them, but it is safer to obtain the solution of this complicated problem from experiment. Two distinct effects are produced during flight: first, the bird is upheld against the force of gravity; second, it is propelled horizontally. Is the bird in the air sustained at a constant elevation, or is it rather subject to oscillations in the vertical plane? Does it not exhibit, by the intermittent effect of the strokes of its wings, a series of ascents and descents, the frequency and extent of which cannot be observed by the eye? Is not the bird also subjected to a variable velocity in its horizontal course? Does it not receive a jerking motion from the action of its wings? These questions can be

solved by experiment in the following manner: Since we possess the means by which distant motions produced by pressure exerted upon a drum filled with air are made to record themselves, we must seek to connect the movements which we would study with a pressure of this kind. The oscillations which the bird executes in the vertical plane should be made to produce alternately strong or feeble pressure on the membrane of the drum, according as the bird rises or falls. The same should be done in seeking the variations of its horizontal velocity. Suppose that a flying bird carries upon its back a light metallic drum, like the one already described; that the membrane of this drum be turned upward, and that this instrument be put in communication with the registering apparatus by means of a long tube. If the membrane of the drum freely partakes of the motions of the bird it will not produce any displacement of the air in the apparatus, and the registering lever will remain motionless. But if we prevent the membrane from partaking of all the motions of the bird, if we can give it a tendency to remain at rest while the drum is moved, motion will be produced in the air with which the drum is filled, and the signals will be registered by the lever. Now, we can produce this tendency to remain at rest upon the membrane by loading it with an inert body, such as a disc of lead.

Fig. 29 shows the drum with an inert mass upon its membrane. This mass is formed of discs of lead, of which a certain number can be added or taken off, until the apparatus responds satisfactorily to the motions of vertical oscillation imparted to it. In this arrangement the movements in the horizontal plane are without influence upon the apparatus. If the drum is suddenly raised, the inert body, not participating in this elevation, depresses the membrane exactly as if the mass itself had been depressed and the drum had remained motionless. Conversely, when the drum descends, the inertia of the mass resists the motion as if it or the membrane had been raised and the drum had remained motionless. We may remark that the movement of the lever is in the same direction as that of the drum; that is to say, if the drum be raised the lever also raises itself. It may happen with an apparatus of this kind, that in the motion of the wings rubbing may be produced on the membrane of the drum which will make confusion in the signals. To avoid this I cover the upper part of the apparatus with a metallic network, as seen in Fig. 29. The drum is there represented in the hand, held by the transmitting tube connecting with the registering apparatus. If the drum is moved in the vertical plane the lever is seen to move in the same direction, at the same instant of time, and with an amplitude proportionate to the motions of the hand. If, on the contrary, we give the mass a lateral motion, no effect is produced upon the lever and no signal is made. But it may be said that an inert mass placed on an elastic membrane tends to execute vibrations peculiar to itself, and that the apparatus will transmit these vibrations of the mass of lead and the membrane which carries it independently of the oscillations of the bird. How shall we get rid of this complication? The law of vibrations teaches us that the duration of the double period of each of them varies with the weight of the vibrating body and with the elastic force of the lamina which carries it. The greater the mass and the feebler the elasticity

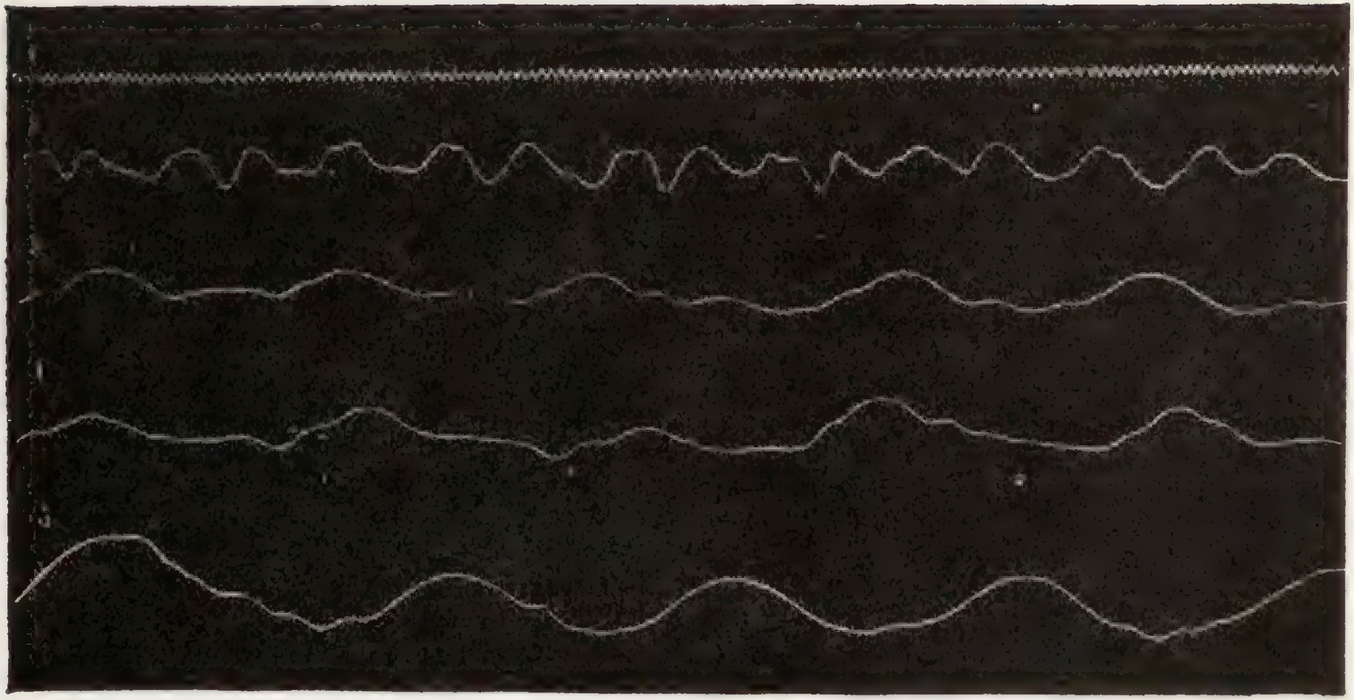
the longer will be the period of vibration. Now, the motions which we are studying are rather frequent, some birds making eight or ten strokes of the wing per second. If we arrange it so that the period of oscillation of the mass of lead itself is much longer than that of the bird, we shall no longer be troubled by the complication of these interfering motions. By employing a heavier mass and a less tense membrane, a good trans-

Fig. 29.



Apparatus for transmitting to the registering lever all the oscillations imparted to it in a vertical plane.

Fig. 30.



Line 1. Chronographic trace of a tuning-fork vibrating 100 times a second. 2. Vertical oscillations of the wild duck during flight. 3. Oscillations of the hen-hawk. 4. Of the screech-owl. 5. Of the harrier.

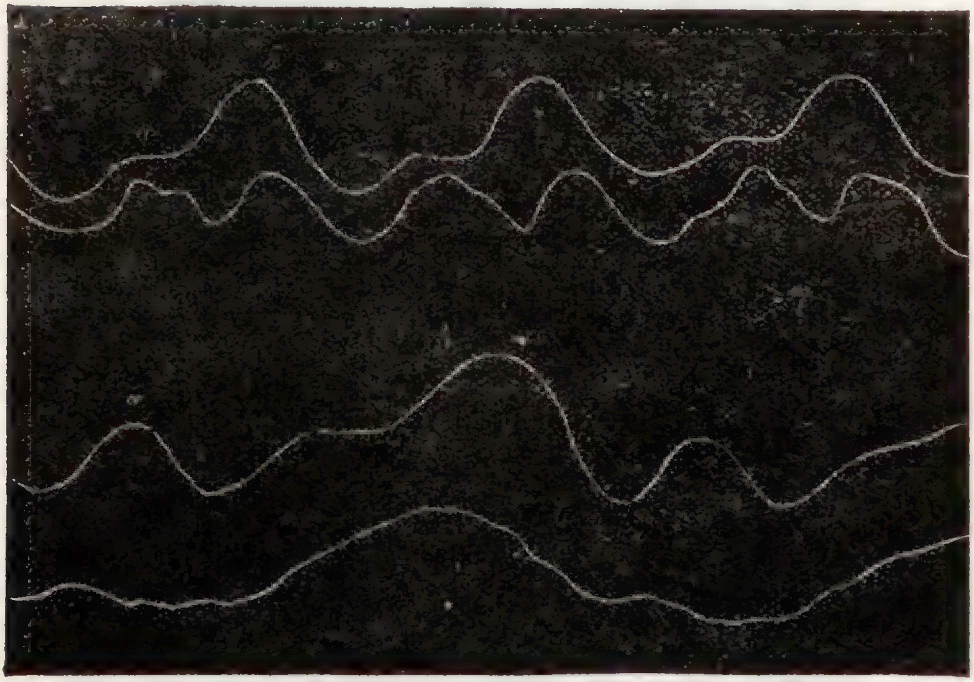
mission of motions, which are not too slow, may be obtained, for instance, such as last less than half a second. It is not necessary, either, that the instrument should be applied to the study of the oscillations of all species of birds.

But to make sure of the accuracy of the apparatus it should be verified by the method much like that which I have used to correct all my apparatus. This consists in making directly, by hand, the tracing of the motion which I have imparted to the weighted drum, and observing whether the registered motion was the same as the first.

Experiments made upon different kinds of birds, ducks, harriers, hen-hawks, and owls, have shown me that, in relation to the intensity of the oscillations in the vertical plane, very varied types of flight exist.

Fig. 30 shows tracings, furnished by different kinds of birds, upon a cylinder turning at a uniform rate, and contrasted with a tracing produced by a tuning-fork making 100 vibrations per second. These tracings enable us to estimate the absolute and relative duration of the oscillations of flight in these different birds. It follows from these figures that the frequency and amplitude of the vertical oscillations vary a good deal with the kind of bird under consideration.

Fig. 31.



In the upper half is seen superposed the muscular tracing and that of the vertical oscillations in a wild duck. Below the undulation *a*, which indicates the elevation of the wing, is seen a vertical oscillation; and another, below *b*, which indicates the lowering of the wing. In the lower portion are the same tracings obtained from a harrier; here the oscillation at *a*, which corresponds to the elevation of the wing, is less marked than in the duck.

To better comprehend the cause of these variations, let us register at the same time the vertical oscillations of the bird and the action of the muscles of its wing. If we make this double experiment upon two birds, differing in their manner of flying, such as the wild duck and the harrier, the tracings represented by Fig. 31 will be obtained.

The duck presents two energetic oscillations at each revolution of its wing; the one at *b*, at the moment when the wing relaxes, is easily understood; the other, at *a*, at the moment when the wing rises. To explain the ascension of the bird, during the time of elevation of the wing, it seems to me indispensable to call in the action of the boy's kite, previously alluded to. The bird, moving forward with acquired velocity, presents its wings to the air in an inclined position similar to that of the kite, and thus transforms its horizontal force into an ascending one.

The flight of the harrier presents the ascension which accompanies the elevation of the wing in a smaller degree. May not the cause of this difference be recognized as a smaller relative inclination of the wing toward the horizon?

Determination of the different phases of the evolution of the wing to which the vertical oscillations correspond.—The interpretation of these curves throws light at once upon the experiments made on the variations of the transformation of velocity in the bird, at different moments, during the evolution of the wing.

But, before going further, we may remark that the preceding experiment furnishes a very precious lesson in the theory of flight. In fact, if the bird excutes a series of ascents and descents, the duration of the descending period will approximately inform us of the amount of the positive work which the bird must perform to rise again to the height from which it fell, and we see that the duck, which makes nine vibrations of the wing per second, executes two vertical oscillations during each vibration, or eighteen in a second. Each oscillation is composed of a rise and fall, so that each descent of the bird cannot last more than one thirty-sixth of a second. Now, if we subtract the effect produced (as in a parachute) by the outspread wings of a bird, we find that a body which falls during one thirty-sixth of a second traverses only fifty-two millimetres. This fall repeated eighteen times a second constitutes a total rise of 9·36 centimetres, necessary to maintain the bird in the same horizontal plane during one second.

In the tracing of the harrier, the descents are less than in the wild duck, probably on account of the large surface of the wings of this bird.

Determination of the variations of the rapidity of flight.—The second question to be solved relates to the determination of the various phases of rapidity of flight. The solution can be found in the following manner: If the weighted drum be placed upon the bird's back in a vertical plane perpendicular to the direction of flight, it will be insensible to vertical oscillations, and will only indicate those of forward and backward; also, by turning the membrane of the drum forward it is clear that if the advance of the bird is accelerated, the retardation of the weight on the translation of the apparatus will produce a crowding of the air in the second drum, and an elevation of the registering lever, while a relaxation of the effort of the bird will bring about a descent of the registering

lever. Experiments upon the kinds of birds previously mentioned furnish tracings analagous to those of the vertical oscillations. If it is true, as I suppose, that the vertical oscillation of the bird at the moment of raising the wing be due to the upward transformation of velocity, by obtaining, simultaneously, the tracing of the vertical oscillations and those of the variations of velocity, we shall have the means of confirming this theory. When obtaining at one time the two kinds of oscillations in the flight of a harrier, I have seen that the phase of descent of the wing resulted both in the elevation of the bird and the acceleration of its speed. This effect is the necessary consequence of the inclination of the plane of the wing at the moment of its descent, as we have previously shown in the flight of insects. As for the phase of elevation of the wing, it is proved that during the slight ascension which it produces the speed of the bird is diminished. In fact, the curve of the variations of rapidity falls as soon as the bird begins to rise. This is, then, a confirmation of the previously suggested theory of the upward transformation of the speed of birds. Thus by this mechanism the descending stroke of the wing creates the force which produces the two oscillations of the bird in the vertical plane. The downward stroke directly produces the ascent which is synchronous with it, and indirectly by creating the velocity which prepares for the second vertical oscillation.

Simultaneous tracing of the two kinds of oscillation of the bird.—Instead of representing each kind of oscillation separately, I have thought that it would be more instructive to obtain a single line which, by its curves, should represent both of the movements which the body of the bird executes in its course through space. The method which has been used to obtain the curve of the point of the wing, with some modifications, can be made to furnish a simultaneous tracing of both kinds of motion. For this both drums must be connected with the same inert mass, and placed at right angles to each other. Turning back to Fig. 23, which shows the two levers connected by tubes which transmit to the one all the motions executed by the other, when any motion is imparted to the first lever, the second lever reproduces the same motion in the same direction. Now, let us charge one of the levers with a mass of lead, and, taking the support of the apparatus in the hand, make it describe some motion in a plane perpendicular to the direction of the lever. We see that the lever No. 2 executes directly opposite movements. In fact, since the motive force which acts on the membranes of the drums is simply the inertia of the mass of lead, and since this mass is always behind the motion given to the apparatus, it is clear that if the whole be raised the mass will keep the lever down; if the whole be lowered, the mass will raise the lever; if it be carried forward, the mass will hold back the lever, &c. Now, the second lever, executing the same motions as the first, will give curves which are directly the opposite of the motion which has been given to the support of the apparatus. This being settled, now for the experiment:—For this I take the apparatus represented on the back of the harrier in Fig. 25; I remove the rod which receives the motion of the wing, and the parallelogram which transmits it to the lever. I keep only the lever connected with the two drums and the mounting which attaches it to the bird's back. I fix a

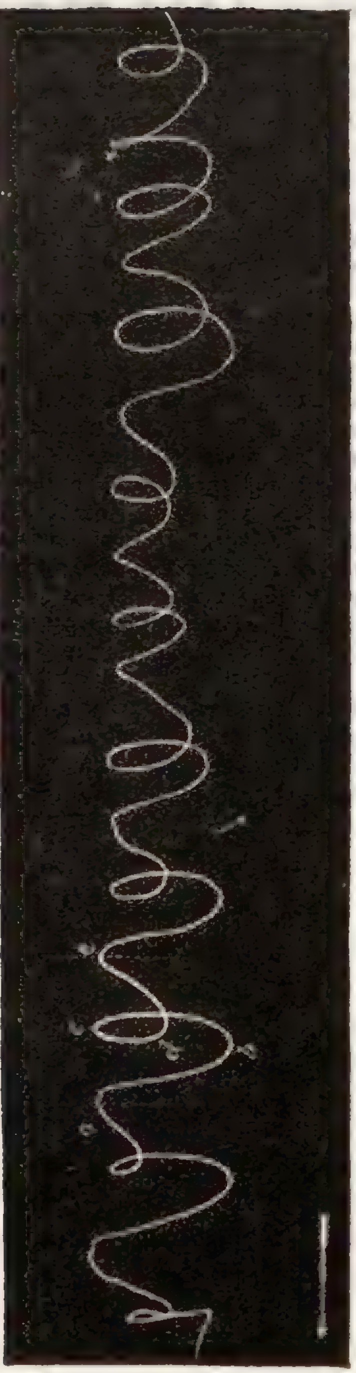


Fig. 32.

Simultaneous tracing of both kinds of oscillations executed by a harrier during flight.

mass of lead on this lever and let the animal fly. The tracing obtained is represented by Fig. 32.

The analysis of this curve is at first sight extremely difficult. I hope, however, to succeed in showing its signification. It is traced on the cylinder under the same conditions as Fig. 26, showing the different motions of the point of the wing. The glass plate moves from the right to the left; the tracing is read from left to right. The head of the bird is toward the left; this flight is in the direction of the arrow. We can divide this figure by vertical lines passing through homologous points, cutting it either at the top of the loops or at the summit of the simple curves, as represented at the points *a* and *e*. Each of these divisions encloses similar elements, although their development is unequal in different parts of the figure. For the present we shall neglect these details.

It is evident that the periodical return of similar forms corresponds to a return of the same phases in an evolution of the bird's wing. The division *a e* thus represents the different motions of the bird during an alar evolution.

Let us recollect that in the curve which we are analyzing all the motions are the reverse of those which the bird really executes. The two vertical oscillations, the great and the small, should then be represented by two downward curves. It is easy to recognize them in the great curve *a b c* and the small curve *c d e*. Thus the bird rises from *a* to *b*, falls from *b* to *c*, again rises from *c* to *d*, and re-descends from *d* to *e*; but these oscilla-

tions encroach on each other, producing the loop *cd*. The oscillation *cde* partly covers the first anteriorly. This is a proof that the indications of the curve are the reverse of the true motion; for, at this moment, the bird recedes, or at least relaxes its course. As the apparatus is only sensible of changes of velocity, it is clear that the tracing does not take the uniform rapidity of the bird into account, but indicates acceleration as a forward movement and retardation as a retrograde movement. This figure, then, sums up all the preceding experiments which we have made on the motions of the bird in space. It is here seen that the bird at each evolution of its wings rises and falls twice successively; that these oscillations are unequal; the larger, as we know, corresponding to the depression of the wing, the smaller its elevation. It is also seen that the ascent of the bird during the raising of the wings is accompanied by a retardation of its speed, which justifies the theory by which this ascent has been considered as made at the expense of the bird's acquired velocity. But this is not all; this curve also shows us that the motions of the bird are not the same at the beginning and end of flight. We have seen already (Fig. 20) that the first strokes are more extended than the others; we now see that at first—that is, at the left of the figure—the oscillations produced by the descent of the wing are also more extended. But theory foretold that the oscillation of the elevation of the wing being derived from the acquired speed of the bird should be very feeble at the beginning of flight when the animal has acquired but little impetus. The figure shows us that this does happen, and that at the beginning of flight the second oscillation (which forms the loop) is very insignificant.

At last, then, we are in possession of the principal facts upon which the study of the mechanical power developed by the bird during flight can be established, and we see that it is during the descent of the wing that the entire motive force which sustains and directs the bird in space is created.

CONCLUDING REMARKS.

One of the most important events in connection with Aëronautics during the past year has been the trial of M. Dupuy de Lôme's navigable balloon. This balloon was constructed by M. de Lôme for the Government of National Defence, at a cost of £1600., and was intended to open a communication between Paris (then besieged by the Prussians) and the departments. But, owing to unavoidable delays, it was not finished until just four days before the capitulation. Then came the Commune, and all the disorganization which followed it; and it was not till the 2nd February, 1872, that M. de Lôme was able to ascend on a trial trip from Fort Neuf at Vincennes.

Before describing the balloon and the ascent, it may be as well to say a few words as to the enu which the eminent engineer proposed to himself. He did not pretend to be able to successfully contend with the wind, but only to deviate from the direct set of the wind when running before it; so if the wind set straight from Paris to Brussels an ordinary balloon could only land at some point between Paris and Brussels, but with M. de Lôme's balloon the aëronaut might deviate from the wind's course, and descend at London or Cologne as he saw fit.

The following is a description of the balloon as given in M. de Lôme's report read before the Academy of Science. The form of the balloon is oval, its diameter being about wo-fifths of its horizontal length.

Total length from end to end	118ft. 6in.
Diameter at the point of greatest circumference.	49ft. 2in.
Diameter of the screw	29ft. 6in.
Number of blades	2
Pitch of screw	26ft. 8in.

The rudder is a plane triangular surface, made of unvarnished calico, and is kept in its place by a horizontal beam six metres long at its lower extremity. It can turn easily on its forward extremity. The height of the rudder is 5 metres, and it has a superficies of 15 square metres. The car is of wicker-work, and of sufficient size to contain comfortably the windlass for the screw, and eight men to work it; the ventilator with which to manage the small balloon (we shall have to speak of this presently), and the man who attends to it. In all, fourteen persons can be carried. The driving screw is directly carried by the car. The shaft of the screw is a hollow steel tube. This shaft is constructed so as to allow of the screw being easily dismounted when a landing is effected. The rudder is fixed to the balloon itself, and the screw, as we said, is below it, and immediately attached to the car. Two blades only are used in the screw instead of four, because when the ground is touched the two blades can be placed horizontally, so as to escape injury. Were there four blades, the screw would be almost certain to be broken at every landing. The windlass which turns the screw is worked by four, or, if necessary, eight men, in a similar manner to the steering wheel of a ship, only the wheel is placed parallel to the axis of the car, instead of at right angles to it, in order to lessen the rolling occasioned by the movements of the men working the windlass.

The material of which the envelope of the balloon is composed is white silk, weighing 52 grammes, not quite 2oz. to the square metre; and a coarser lining weighing 40 grammes the square metre, and seven coats of india-rubber, which together weigh 180 grammes, a little over 6oz. the square metre. Thus the whole weight of the external web of the balloon is 272 grammes, about 9oz. to the square metre. In order to render the web of the balloon totally impermeable to the hydrogen gas with which it is inflated,

the silk was painted over with a sort of gelatinous compound, invented by M. Dupuy de Lôme. The total weight of the two balloons when ready to start was 570 kilogrammes, or rather more than half a ton. The web of the balloon was reckoned to be capable of supporting a pressure of over 2000 pounds to the square yard. The smaller balloon is, more correctly speaking, only a portion as it were of the larger balloon. It is formed by means of an inner skin, separating the bottom of the balloon from the rest. This compartment occupies about one-tenth of the whole capacity of the balloon, and serves to keep it stiff and of the required shape. By these means M. Dupuy de Lôme has attained the two ends he proposed to himself, viz., first, permanence in the shape of the balloon; and, secondly, an axis unquestionably parallel to that of the force of propulsion.

M. de Lôme calculated that the resistance to the balloon at a speed of 7ft. 5in. per second, or 8 kilometres an hour, would be 25lbs., and that this speed could be obtained by 21 revolutions of the screw per minute.

We will now describe the ascent:—There was half a gale of wind blowing at the time, and the screw had been slightly damaged. The inventor did not hesitate, however, to make the ascent. The end justified his confidence, for not only was he able to land near Noyon, in the Department of the Oise, some seventy miles north-east of Paris, but his balloon more than answered his expectations. The screw, when worked by four men, drove the balloon 8 kilometres (about 5 miles) an hour quicker than the rate at which the wind was blowing. By the use of the rudder the course of the balloon could be altered 11 degrees either way from the set of the wind, making a total deviation of 22 degrees. The screw when worked by eight men drove the balloon at the rate of 10½ kilometres per hour. The

number of revolutions at this speed was 27½ per minute, and the power required was 26,400 foot-pounds per minute. The slip of the screw was 24 per cent. Although the speed obtained was not great compared with the velocity of an ordinary wind, yet by employing an 8-horse power engine in place of the eight men, a speed of 22 kilometres per hour would have been obtained, which would enable the balloon not only to deviate from the wind, but to struggle against it when moderate.

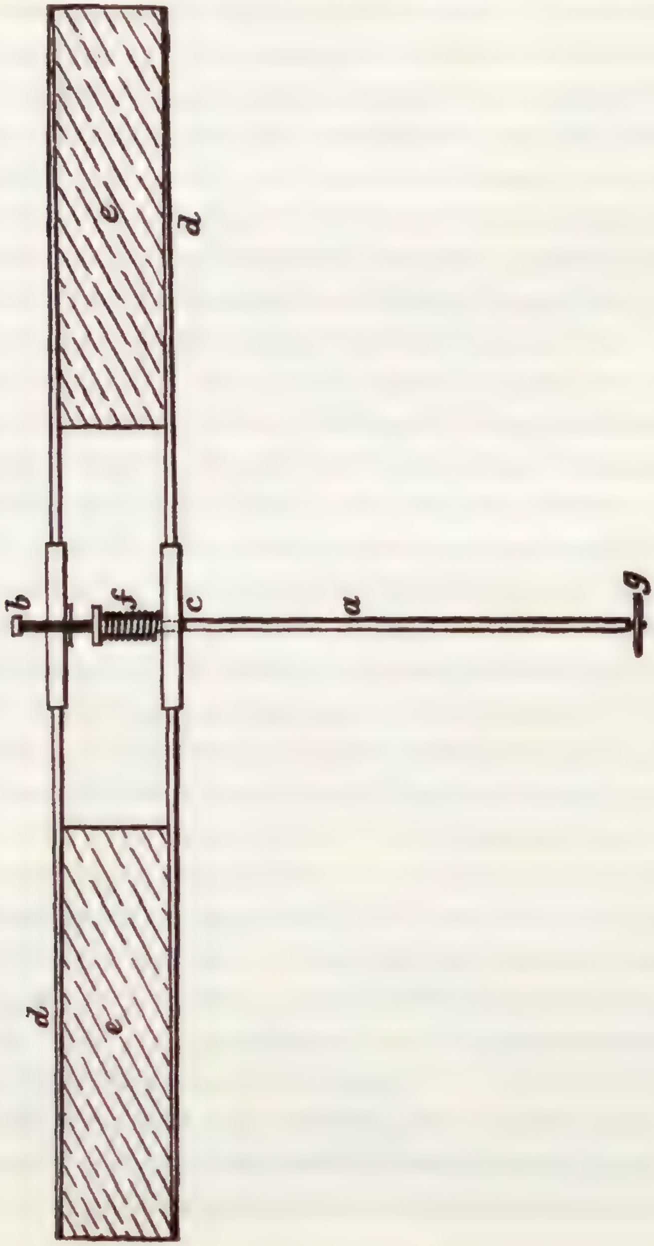
Experiments with aërial screws have occupied attention during the past year. One correspondent, Mr. Lingfield, has constructed a piece of apparatus consisting of two superposed screws, rotating in opposite directions; he found that there was no advantage in using four blades, but that an equally good or better effect could be obtained by means of two blades by which he caused a lifting force of 14¾lbs. by his own muscular strength. Having a suspicion that the friction of the surface of the fabric absorbed a considerable per centage of the power, he pasted tissue paper over the calico of the vanes, and thus increased the lifting force to 18lbs.

This proves the importance of attending to the question of friction in aërial mechanism; to diminish it as far as possible on the surfaces of supporting planes gliding on air, and in reciprocating or oar-like propellers, when possible, to utilize friction as an aid in gaining additional abutment or hold on the air, a principle probably made use of by some birds.

Similar experiments have also been made in Paris by which a lifting force of 26½lbs. was obtained. But these results, though obtained by independent experimenters both here and on the Continent, must not be taken as conclusive of a maximum effect, for probably a far higher reaction or force against gravity may ensue from more suitable forms of screw, and in the best means of giving them motion.

One difficulty has been a ready means of varying the angle

or pitch of the screw, in order to suit the velocity of rotation and the force applied. Mr. Wenham has proposed a simple kind of screw for this purpose, constructed in the following mannner :—*a* is a hollow spindle or tube, at the end of which



is fixed a cross-socket *b*, with two arms. Sliding on the spindle loosely is a similar socket, *c*. Into the ferrules of these two pairs of sockets, taper flexible wands, *d d*, are thrust; these are shaped like billiard cues, and made of light elastic wood. From the extremities of these to near one-third the distance towards the centre a piece of fabric, *e e*, is sewn between them. A light iron rod passes through the hollow spindle, having a short cross arm at the outer end. Two return rods from this, afford the means of compression to a spiral spring *f*, surrounding the spindle, and resting on the sliding socket *c*. At the lower end of the spindle there is a cross-handle *g*, tapped to receive the screwed end of the inner rod. By turning this handle the spring is compressed, forces down the lower sliding socket, and of course gives any required tension to the fabric connecting the rods or arms of the screw. In this condition the four arms and planes of the fabric coincide with the axis, but if this is set in rotation, the two lower arms and socket being free thereon, are forced back by the resistance of the air, giving an inclined position to the fabric of the proper form for an aërial screw, with a somewhat hollow face or expanding pitch, which can be exactly determined by the tension given to the spring; if this is slack the pitch will be a fine one, and when screwed hard up the lower socket will yield but little, and a coarse pitch be obtained.

As the rods twist or deviate from each other, of course the connecting distances between them become greater at the extremities than near the centre. This is compensated for—1st, by leaving the middle as an open space; 2nd, by having the fabric loose at the extremity, so as to meet the coarsest pitch required; and 3rd, the rods being properly elastic at the ends, yield so as to stretch the fabric uniformly in fine pitches, giving the blade of the screw a taper form, which is not an objectionable one, but the reverse.

The great advantage of this self-compensating aërial screw is its portability. The rods may be pulled out of the sockets, and rolled up together with the fabric as one piece in a compact form.

There is a peculiar feature connected with the working of this Society to which it may be as well to allude, viz., its apparent inactivity.

The work which is surely being accomplished is effected under a variety of conditions by private individuals, but almost always under circumstances of discouragement within the experimenter's private circle.

In these cases the moral support of the Society is considerable. The Council feel that of theory we have had almost enough, and that however much the publication of the Papers read at the General Meetings may have cleared up some of the apparently insurmountable difficulties attending the subject, the continual expression of opinion is liable to become rather wearisome.

We now require and look for facts, and for these we would wait before we call upon members to discuss them.

The Council perceive that those of the members who are not actually engaged in experiments perfectly acquiesce in this view by the patience with which they wait the very few Public Meetings of the Society.

It is not, however, in these Meetings that the real business of the Society is effected. The Secretary has a large correspondence, and the calls upon his time in interviews, both at home and abroad, are more than could be expected from any one less interested in the subject.

It is the knowledge of this which induces a few members of the Council to render all possible aid by meeting for consultation and in furtherance of the attainment of results.

Dr. W. Smyth wishes in this number to make the following remarks relative to a Paper read by him and printed in the Second Annual Report. He feels it the more necessary because of his statement having been quoted by various authors. "After reflection upon the experiments performed by me in dividing the nerves of the wings of pigeons, I am of opinion that they were inadequate to determine whether the pigeon could fly or not with all sensation severed. The experiments were hastily performed for a coming Meeting of the Society, and I judged it to be as reported at the time, but as the experiments are being quoted by others I desire their actual value to be correctly known."

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THE FOLLOWING

SPECIFICATIONS OF PATENTS.

Date.	*No.*	*Subject.*	*Patentee.*
1872	411	An Aërial Machine	D. S. Brown
„	821	A new or improved Balloon Locomotive, or Navigable Balloon	Matthew Augustus Toul.
„	3076	A new system of manageable Balloon called Duthu's system, applicable to the management of Balloons ...	Jean Baptiste Duthu.

Eighth Annual Report

OF THE

AËRONAUTICAL SOCIETY

OF

GREAT BRITAIN.

FOR THE YEAR 1873.

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Ladies may become Associates upon the same terms.

Eighth Annual Report

OF THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN,

FOR THE YEAR 1873,

Containing an Account of the Proceedings, and a Selection from the Papers and Communications received by the Society during the year, with concluding Remarks upon the present state of the Science.

THE Annual General Meeting of Members of this Society was held in the Rooms of the Society of Arts, on Monday Evening, the 30th of June, 1873. Mr. JAMES GLAISHER, F.R.S., presided. Several models and specimens of apparatus were on the tables, and were exhibited in the course of the evening.

The CHAIRMAN, in commencing the business of the Meeting, said: Ladies and Gentlemen,—We meet again hopefully, as we have met on previous occasions. We have to speak to-night, I think, of some progress; our progress has been for some years very slow indeed. Whether it has been much accelerated during the past year I cannot say, but still I think a decided improvement has been effected. It has been a year distinguished from previous years by experiments, and it is to experiments we must look for any ultimate success that may attend our efforts. As I have before said, those experiments that are essentially necessary and strictly applicable to aëronautical navigation have this great

advantage—that, even should we not succeed in our hopes, our knowledge is increased in the direction required, and in a direction which may be useful in providing for the wants of man Therefore I have urged the continuance of these experiments. because good must follow from them though the ultimate object may or may not be attained.

Perhaps I may be permitted to say a few words on some of the attempts that were made last year. In the order of time that of M. Dupuy de Lome should be first noticed. M. de Lome attempts to cause a balloon to deviate from the direction in which the wind blows, and his invention is one to which some attention should be paid. Two conditions, he said, must be complied with in order to achieve his purpose. The first is the permanence of form of balloon; and the second is that the least resistance should be in a direction parallel to the propelling force. The weight of the balloon in which he made the experiment was about a ton and three-quarters, but, when everything was ready, the weight was increased to four tons. The screw was worked by manual labour. M. de Lome has made experimental trips with this balloon; and he states as an absolute fact that he did cause a deviation from the normal direction in which the wind blew of as much as twelve degrees on the one side and twelve on the other. That is a great achievement. What a blessing it would have been if such an invention had been in operation at the siege of Paris! We feel grateful that a power did exist to give us news from the interior of the city; but if this discovery had then been made we could have sent news in as well as got intelligence out. M. de Lome employed seven or eight men to work the screw. A small engine would have done better and would never tire. However, in what has been done, there is evidence of a distinct advance.

The second invention I have got to mention is that of Paul Hanlein, who has made a gas engine for balloon

propulsion,—to propel the balloon against the wind. On the construction of this engine, and also of the balloon, we have had a good deal of correspondence; and we were told in reply to our enquiries, that both had been tried, and been started from a given place and brought back again. The Aëronautical Society of Vienna have, however, dispensed with the services of Mr. Hanlein, and we have been unable to ascertain the reasons. Mr. Moy, who has just returned, could not tell me. The Vienna Society have constructed a balloon for themselves at an expense of £1200., and they have now taken advantage of what two of our members have done, and have ordered a four-horse power engine which I am told is to be the lightest engine in proportion to its power ever yet constructed. May it be so, because if it should be so we shall have made a great and good step in the direction in which we are working. With regard to balloon propulsion, for my own part knowing how completely at the mercy of wind I have been in balloons, I can hardly think any power can control them. They may vary a little right or left, but the wind must always have power over a balloon, especially if it is a large one. I have had enough experience to feel certain of that. We have great satisfaction in knowing that the long-desired engine has been obtained, and that at length we shall be enabled to aid the balloon by an engine of lightness, power, and safety. I was asked by Mr. Brearey whether I would go into a balloon with one of those engines. I said that if the bottom of the balloon were closed, so that the gas could not come out, I would go. But for purposes of experiment six feet from the ground would be quite as effectual as sixty.

Mr. Brearey remarked that the engine was intended to aid in ascent and descent only.

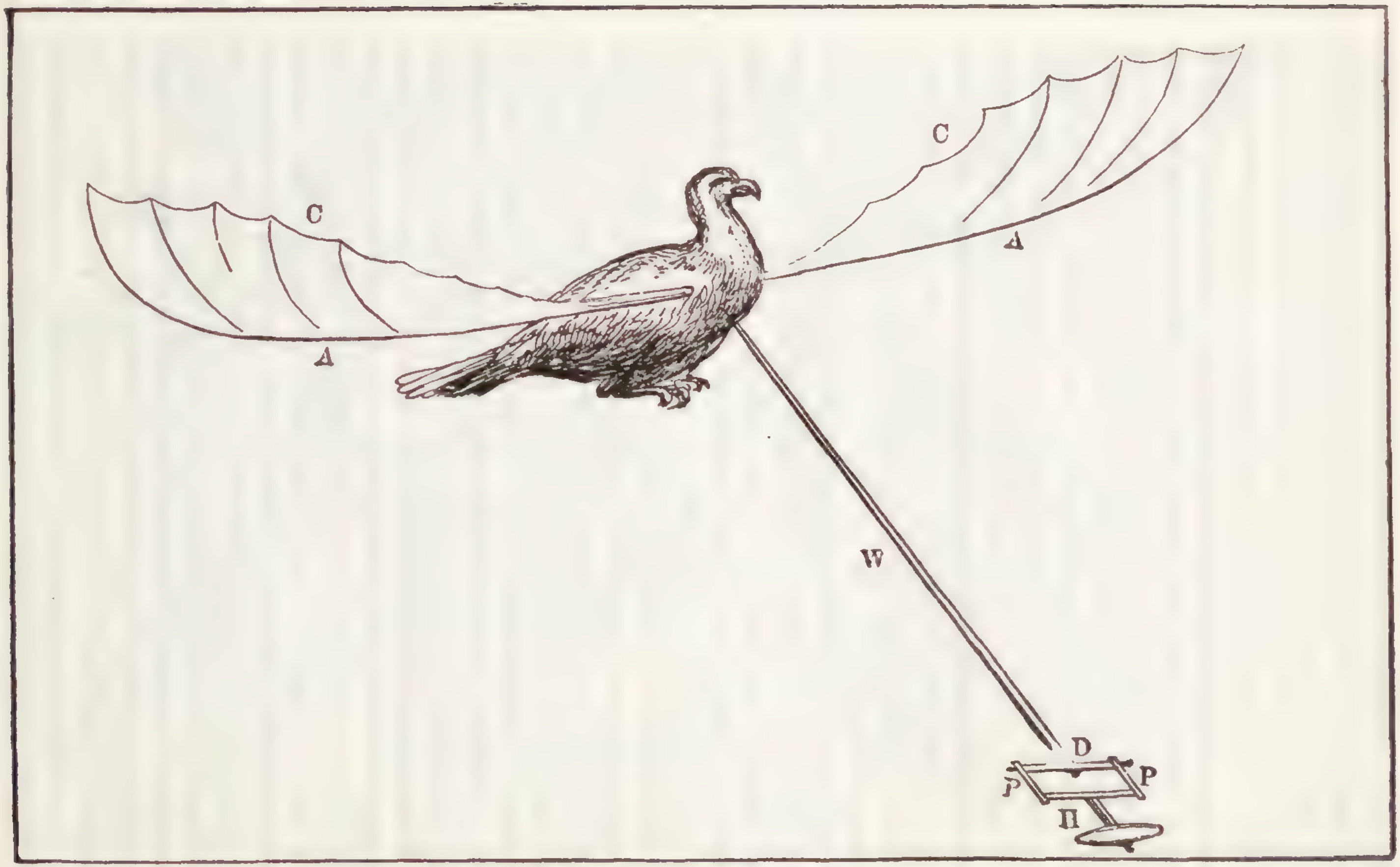
The Chairman: Well, let us do everything from the beginning. Ascent and descent is all we can try at the

present moment. These Gentlemen (Messrs. Moy and Shill), members of our Society, who have often spoken here and given us the benefit of their labours and attention, have now taken out a patent for this engine; and all I can say is, God speed them, and may they be successful (cheers). I will not longer occupy your attention, because there are several papers to be read. I can only say for my own part that the fact that the results of investigation in one direction should have produced an engine of this kind, shows that the application of our exertions is not at all limited by the objects for which those exertions are intended. Here is a certain object attained. Herschel, when he examined two small stars close together, with the view of obtaining the parallax of the brighter, and found that one went round the other, left his original object of enquiry while he pursued the new investigations thus suggested. So these gentlemen, having started in one course, will now follow that which may be most beneficial to mankind at large (hear, hear.)

Mr. F. W. Brearey, the Hon. Secretary, read a communication from Mr. Artingstall, of Manchester, as follows:—

Sir,

The most difficult problem to solve respecting the flight of birds is the question how does a bird maintain flight without progression, like the kestrel hawk, &c.? Even the heavy and slow winged heron must be able to hover, or it would be utterly impossible for it to alight, as it does, so gently on its nest and eggs, and dispose of its long shanks. It must also leave its nest equally as gentle. This totally disproves the common assertion that a large and heavy winged bird is obliged to run before it can get on the wing, or else start from some eminence. The fact is that no bird could alight on a particular bough or spot without some degree of hovering. But the difficulty of explaining the theory of hovering becomes very great when we know that the muscles which depress the wings of a bird are far stronger than those which elevate them, and also the muscles that depress the back edges of the wings are stronger than their opposing ones; yet the upstroke is as quick as the



The planes of the wings are at right angles with the wand w, excepting the little raising of the back edges (as mentioned) just to catch the air at the beginning of the down-stroke.

downstroke. My decided opinion is that true flight requires the vibrations to be equal like a musical tone, and that the wing is raised by the reaction of its back edge against the air, and of course the reaction of the air against it.

To illustrate hovering, in some measure, I made the following experiment *(see diagram)*:—AA the wings, that is the front or thick edges of them; CC the back or thin edges of them; W a wand or stick about the size and length of a common ½-inch walking stick, one end of which is attached to the wings at B, and the other to the centre D, that moves on the pivots PP, so that no support can be given to the artificial bird but the air; H is a handle like the handle of a common corkscrew; E represents the body of the bird, and is made from very thick cardboard; it is more for ornament, or to show the direction of flight, than for any real use. The expanse of the wings is 3ft. 9in. from tip to tip; their breadth 12in.; the weight of the bird and wings 8oz., that is without the handle and wand.

Now it will be evident that if the handle H be grasped, and a motion given to it so as to twist the wand W backwards and forwards, it will cause the wings to vibrate like the balance-wheel of a timepiece. Now if the wand W be placed at an angle of 45° with the horizon, then the plane of the arc of vibration of the wings will be at 45° with the horizon; therefore the wings will not strike directly downward, but at an angle of 45°, and rise in the same angle. The wings are fixed with their thin edges CC slightly raised above the plane of the arc of vibration, so that at the beginning of the downstroke the air may catch underneath the surface of the wing and continue to bend its back edge upwards until the termination of the downstroke. Now when the wing rises, its back edge reacts downwards, so this stored-up power is returned, and continues the buoyancy of the artificial bird during the upstroke without any motive power being expended. It will be seen in this experiment that the wings do not act independent of each other like birds' wings, but as though they were at each end of a balance. From this it might be supposed that constant buoyancy is only maintained because one is in full power whilst the other is ascending. That such is not the case is proved by stripping the covering off one wing and retaining the "bare poles" or skeleton merely to balance the other, yet this one wing maintains constant buoyancy, but it requires much more dexterous manipulation to produce uniform pulsations as the balance of resistance is disturbed.

In addition to the reaction of the back vibration in raising the wing, the elasticity of the wing-stems AA I believe assist the flight, as they are

bent in the downstroke and return to their neutral position during the upstroke. Perhaps the elastic torsion of the rod W may play a part in producing constant buoyancy. After all there is something at present inexplicable about the action of this apparent toy; but one thing is certain that, if the model be well made and nicely handled, it will hover beautifully at the end of the wand W with a silent gentle and equable motion, as the poet would say, like the soft flutter of an angel's wing. Certainly very unlike many attempts that are being made to produce flight by might and main force with their violent and useless flapping, wafting, skimming, screwing, &c., what may be called trying to fly with a vengeance without first getting to know, by experiments, the true theory of flight and the power required.

I may observe that elasticity plays a primary part in the foregoing experiment, just to prove the existence of a great principle; but in a bird's wings elasticity is only secondary, for ten times better effect is produced by the beautiful and wonderfully organized play of the muscles, both of the thorax and the wing itself, which develope the wonders of flight, and enable first class flyers to fly with very little expenditure of *motive* power.

From my numerous experiments and observations I am now thoroughly convinced that man possesses far greater muscular power than is requisite for tolerably swift horizontal flight. According to the principles I have laid down (see 5th Annual Report of the Aëronautical Society of Great Britain, page 36), all that is required in *swift* horizontal flight to *maintain buoyancy* is a very slight and *imperceptible* direction of the *propelling* force of the wings *upwards;* this I call the ANGLE OF BUOYANCY. If the wings propelled *quite* horizontally, the bird would soon go to the earth like a military projectile fired horizontally; but this slightly oblique action of the wings upwards simply prevents descent beginning, and that, too, by an almost nominal expenditure of power; but when the bird gradually goes slower the obliquity of the angle of buoyancy increases and is soon perceptible, yet the bird may still continue its horizontal path, but as the progressive speed decreases then, of course, the resistance of the atmosphere to the body of the bird also decreases, but the buoyant power required becomes greater until progressive motion ceases, then the bird must hover to maintain flight at the same elevation, but it generally alights on a bough or elevated spot, for the generality of birds do not like the labour of hovering even for a few seconds

I feel confident that my theory of the angle of buoyancy will be of

vast *practical* importance to aëronautics, particularly as I could now produce wings of great propelling power which could be easily and simply worked by manual power without any complex machinery.

However cheering it may be to think that swift horizontal flight requires very little power (as far as buoyancy is concerned), yet it must be remembered that this is only the *effect* of flight already obtained. To obtain the *cause* or primary flight is the great difficulty in Aëronautics.

I am, Sir, your obedient Servant,

To F. W. Brearey, Esq. F. D. Artingstall.

Mr. Moy thought few persons would understand the paper which had been read. He really could not see the drift of it, though he had paid some attention. The writer spoke of hovering as being a difficult performance. Mr. Moy believed the motions of a bird could be explained on engineering principles, and it was his opinion that if the bird was not head to wind it could not hover. The writer also attempted to prove that the power of alighting gently, proved that no run was necessary at starting, whereas it proved nothing of the sort; the two movements were very different. When the bird was coming down its wing acted as a buffer on the air. The writer then said it was difficult to explain the theory of hovering. That he did not see. He thought it very simple. Then the writer ran down all attempts to produce flight by main force. He (Mr. Moy) would however continue to rely on that until they could do without it. They had experimented, and he believed they had found out the true theory of flight, and on that subject he hoped before another twelve months he would be able to enlighten the Society, and to show that the true theory of flight had been found out and patented more than a year. The writer also stated that he thought man had ample muscular power for accomplishing flight. In his shop he had got a pair of aërial screws, and the utmost weight that a man could lift with them was about 18lbs. No man, he believed, would fly by

his own muscular power. Machinery, in his opinion, would do it. He could not compliment Mr. Artingstall on his paper at all.

The CHAIRMAN said they had long since given up the idea of a man flying by muscular power, and had begun to devote their attention to machinery. All they wanted to have was experiment. In his belief experiments, whatever they might be, would prove useful. He would suggest that the thanks of the Meeting should be given to Mr. Artingstall for his paper.

A vote of thanks was accordingly given.

Mr. D. S. BROWN read a paper on the Aëroplane; embracing its construction, stability, and means of propulsion. Mr. Brown, in the course, introduced several models and various lightly constructed apparatus illustrative of his remarks.

THE AËROPLANE.

IT is more than half a century since Sir George Cayley published the result of his researches in elucidation of the problem of Aërial Navigation by mechanical means, which was followed, several years afterwards, by the celebrated project of Mr. Henson, and since then has also appeared a valuable contribution by Mr. Wenham. Yet, up to the present time, no steps have been taken to give to any of these discoveries a practical value. This is much to be regretted, as it represents so much time unnecessarily lost, and has probably delayed the realization of the most important means of locomotion for more than one generation. Having myself devoted a great deal of time and attention to the subject under every aspect which it has assumed, I will state, as briefly as possible, what I consider the essential conditions to be for achieving success by the aëroplane principle of support. And first, as regards the

CONSTRUCTION.

The membrane of the plane should be as smooth and tight as that of a drum, which may be best effected by fixing it on the frame when in a moist state, or constructing it in parts, or of indiarubber; and if also made double-walled it would serve as a condenser for recovering the

waste steam when steam is used as a motor, which would be necessary on a long voyage. The forward edge of the plane should terminate in as acute an angle as possible; but I have ascertained by experiment that this is not so necessary with respect to the posterior one in air, as is found to be the case in water, on account of the much greater velocity of the former in closing a vacuum. This is so far fortunate, as it admits of the cleaving angle being made more acute; and in the construction of balloons the matter would be of still greater importance. It is generally thought that, on account of the strain to which such a structure as an aëroplane would be subject, its size must be very limited. But this is only true when the greater part of the weight is concentrated at one point. If the load be equally distributed over its surface, the plane will be supported by the air with as little strain as a plank is on water. Still, it might be very desirable that the framework should be elastic, to prevent fracture and diminish concussion in making a descent. It has been remarked "that if no one had ever seen a bird, nobody would believe in the possibility of flying." But not less wonderful is the graceful way which the bird folds its wings close to its body when not in use, so as to form no incumbrance in walking; as well as the exquisite manner in which the feathers are arranged, that even in stemming a gale not one is ruffled or displaced. The aëroplane, however, by reason of its more regular form, affords still greater facilities for rendering it portable, by hinges, joints, sliding tubes, &c.

Position.

The Aëroplane should not be inclined to its path of motion, but its surface form a direct line with it. There will then be no resistance excepting from friction and the forward edge of the frame. The plane can be kept at the same elevation by slightly directing its course upwards, sufficient to compensate for any fall which may take place. Mr. Wenham stated that a rise of 1 in 30 would do, provided the progressive motion were 30 miles per hour, and the plane loaded to the extent of 1lb. to the square foot. Without such rise in its path it would fall at the rate of about one mile an hour, or, without the horizontal motion, at the rate of 15 miles per hour. The failure of Mr. Henson's efforts is, I think, partly to be ascribed to the use of an inclined plane, for when only one is employed there must be a difficulty in maintaining the required inclination, and at a high velocity the resistance from the surface would have to be met by great force. The following diagrams will assist in illustrating what I have said:—

Fig 1

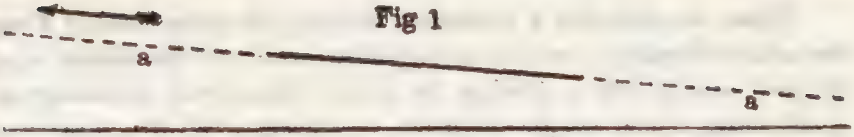


Fig 2

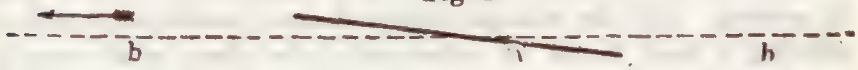


Fig. 1 represents the side edge of a plane, the path of motion of which (*a*) rises slightly. Fig. 2 is a similar view of another plane, the path of motion of which (*b*) is horizontal, and the plane inclined to it.

STABILITY.

This is conferred by motion on bodies in a most striking manner, as tops, hoops, arrows, and many other things illustrate, and the aëroplane will be found to form no exception to the rule. At the same time the importance of properly adjusting the centre of gravity must not be overlooked, and which should be as much below the level of the plane as possible. I have, however, been successful in greatly increasing the stability by employing two planes; one placed before the other at some distance, and both connected by a rod. *c* and *d*, Fig. 3, represent such planes, and *e* the connecting-rod.

Fig 3

These, on receiving a horizontal motion, will glide steadily along like a bird skimming; and the arrangement admits of the planes being inclined, should such a position be at any time found advantageous. A self-acting rudder can also be made, by placing a ball free to move on the plane. Any faulty inclination, such as pitching, would then be corrected by the motion of the ball being communicated to the rudder by a string. And the principle admits of many modifications, such as allowing a liquid to run off either end of the plane, which may be faultily inclined; causing a weight to move backwards and forwards by means of a spring set in motion by the inclination, &c., &c.

Propellers.

Those having an oblique action are best adapted for aërial propulsion, because they are able to overtake the wind or receding current of air caused by the progressive motion of the aëroplane with a velocity as much slower than the wind or such receding current as may be the ratio of their obliquity. It is thus that a bird is able to propel itself so rapidly with a comparatively very slow motion of its wings. For if the obliquity of the stroke be as 1 to 10, every inch which the air is pressed down by the wing will force the bird forward 10 inches. It is precisely the reversed action of a wedge, or similar to a sail set to a side wind, and any one may obtain a practical example of it by observing the slanting manner in which a piece of tin or any plane body sinks in water. As may be expected, the thrust is diminished in proportion to the obliquity of the stroke, but it is compensated for by the slowness of the motion, so that the question of power is not involved. Should a screw be employed, it must therefore be at a very great pitch, although I think that reciprocating planes, acting like the wings of birds or tails of fishes, are preferable. The rocket principle of propelling, on account of its extreme lightness and simplicity, recommends itself. But air discharged from a bellows at so low a pressure as half-a-pound to the inch, has a velocity of 147ft. per second, although by heating it to a high temperature as it escapes the useful effect may be more than doubled. I have some important improvements to suggest as to economizing steam when used for propulsion in this way.

Motors.

Gravity is the most easy force to employ, and large birds always avail themselves of it when they can to obtain their initial velocity, by starting from some elevated spot. "About 60 years ago," according to

Sir George Cayley, "many experiments on a large scale were made by this means, some of the aërial vehicles having 300 to 400ft. of canvas, extended on masts and braced by rigging; and a surface of 54 sq. feet, weighing 11lbs., was found to support 126lbs. in its waft. These trials proved, in a most decided manner, that perfect stability and guidance were attainable. For instance, it was proved that a man placing himself on a machine of proper dimensions for his weight, at the top of a mountain, say one mile above the level of the plain below, might, in calm weather, with steadiness and security, proceed through the air to any place he might choose to steer, about 8 miles in a horizontal direction. Of course the line of flight would be in a continued descent of 1 in 8, gravity being the only cause of the motion of the machine." Impetus, or communicated motion, where the motor is separated from the body moved, is often necessary to obtain initial velocity; and I think this will be more requisite with respect to the aëroplane than any other vehicle. The motion of all projectiles, as well as that of the aërial top, is due entirely to this source. Next in simplicity is spring power; and lightly as it may be regarded, it is by this that the most striking results have as yet been obtained in mechanical flying. True, the machines were only toys, but they carried their motors, although the power was used in the most wasteful manner, so much so, that perhaps no bird of the same weight ever expended so much in the same time. I find by experiment that about 15lbs. of indiarubber cord, stretched to seven times its length, will, in returning to its normal state, yield a power equal to one horse for a minute. It should be placed in the tubular framework of the aëroplane, for it is dangerous to handle when so stretched. The same result might be obtained by placing there instead, 6lbs. of solid carbonic acid, which, without artificial heat, will pass into a gaseous form, and so could be made to work an engine like steam. The objection to spring power is its short duration, but a confined fluid may be made to act as a spring by alternately heating and cooling it, and the force would be thus rendered continuous.

I now come to consider a most important power, namely, manual power, and it will greatly curtail what I have to say, if I state in the first instance that in estimating the power necessary for flight, one of these mistakes so peculiar to the science of Aëronautics is almost invariably made. The one in question arises from supposing that force is necessary to sustain a body in the air as well as to propel it. Now, force is certainly required for propelling, for it implies motion under

resistance. But, theoretically speaking, no power is necessary to support a body, under any circumstances, where no elevation of it takes place. In practice, however, it may amount to almost anything or nothing, according to the conditions observed. In the case of the toys alluded to, which were sustained in one place by the continuous action of screw propellers, it was enormous. Small birds and insects, on the other hand, as I explained in a former paper diminish it to a fraction by intermittent motion; but with respect to large birds where the support is merely the effect or consequence of propelling, no allowance whatever need be made for it. Now, a fair comparison between the locomotive performances of animals in the air with those on the ground will show a result vastly in favour of the former. It is therefore not unreasonable to assume that a man who can propel himself so well upon a velocipede on the ground, would do so still better with a suitable machine in the air. It so happens that the position in which he can exert the greatest amount of muscular power offers also most resistance to the air, and I need scarcely say that such resistance is very different in flying to what it is in walking. It would therefore be necessary for him to work in a narrow compartment, having its front brought to a very acute angle. This would diminish the resistance to about a quarter, and two or three men working in it in a line would reduce the proportion much more.

I will pass the giant motor, steam, by observing that if the weight of the steam engine, or rather steam boiler, ever formed an obstacle to its use for aërial navigation, it has long since been removed by the Society's prize engine, which proved that everything could be brought within 30lbs. per horse power. To say nothing of gas engines, of the explosive and non-explosive kind, where no boilers are necessary, as well as electric ones.

In conclusion, I would suggest that experiments should be made with the aëroplane on a large scale, with a view of ascertaining its locomotive value, as compared with that of a wheel carriage of the same weight on the ground. The initial velocity could be given by a swing or by launching it from a balloon, and the motion afterwards continued by gravity, a spring, manual power, or any other motor that might be advisable to employ. These would solve a number of valuable and interesting problems, such as, whether a man possesses sufficient power for flight, and if not whether flight is practicable at present by mechanical means at all. Even if it should be found that it requires twice as much force as locomotion on the ground, it would not be less economical, provided the journey were made in half the time. In

forming an opinion of the probable success of steam, it should be borne in mind that it has only to exhibit the same superiority over animal power in the air that it has always done elsewhere. The large locomotives formerly employed on the Great Western Railway were capable of working up to 1000 horse-power, although they only weighed 35 tons, including the water and fuel, and the tender 17 tons; which shows a power, in proportion to weight, more than four times as great as that of a horse. To insure as much safety as possible, the experiments could be made above water. And on the subject of safety, I may remark that in locomotion where machinery is employed, nearly all accidents happen by collisions. These are more likely to occur on railways where travelling is on the same line, or at sea where it is on the same plane, than in the air, which is a portion of a sphere, where a thousand aëroplanes may cross each other's paths, at different elevations, and without coming into contact. Even in case of an accident to the engine, the progressive motion of the aëroplane could be continued by gravity until it reached the earth, which it would probably do in a gentle manner as birds are seen to alight. Besides which, springs could be also employed to break the fall. Judging from the theory of M. de Lucy, which is corroborated by the experiments made by Sir George Cayley, as regards the proportion of weight to surface, a plane of 24ft. by 6ft. would be more than sufficient to support a man. This could be propelled with great steadiness by a screw, but much more effectively by the plane itself if its sides were made to move up and down from joints at the centre, like the wings of a bird, in which case the anterior edge of the plane should be rigid, and the other part yielding or elastic. At a distance of not less than 20ft. before or behind the aëroplane should be carried another smaller plane to serve as a tail and rudder, and which should also have affixed to it a vertical one, and the whole moved by an arm turning on a universal joint. The simultaneous elevation of this with the wings and a horizontal fish-tail propeller behind (should there be one), would cause the machine to descend as steadily as a shuttlecock, although too rapidly unless it had a horizontal motion as well, or the aëroplane were made to revolve or to move quickly backwards and forwards. I think, however, that superposed planes are much better. These could be arranged in two groups or sets, each group consisting say of 12 planes 12ft. long and 1ft. broad each, put one above another at 12in. apart; and one group placed before the other at a distance of about 20ft., but connected by a beam. Such an arrangement would combine the greatest sustaining power with compactness and stability.

Aëroplanes may also be wholly made of any airtight membrane, and rendered rigid and even buoyant by inflation.

The Chairman said they must thank any member of the Society who had devoted so much thought and time as Mr. Brown must have given, and they ought to give their thanks to him for having exerted so much patient labour. He had no doubt Mr. Brown would be willing to give further information to those who required it, at the end of the Meeting.

A vote of thanks was passed to Mr. Brown.

Mr. F. W. Brearey announced that Mr. Bennett had been engaged in experiments, and would exhibit the results.

Mr. Bennett introduced an Aëroplane invented by a Frenchman, to be worked by a screw by motive power derived from elastic springs. The great feature about it was the balancing tail, which was regulated by the oscillating motion of a weight. The apparatus was thrown up in the air, and flew with good effect across the room.

Thanks were given to Mr. Bennett for his services.

Mr. Moy was then called upon to describe his engine, which was fixed on a table in front of the Chairman, and occupied less than a cubic foot of space. He began by stating that 100lbs. pressure could be got up in a minute and three-quarters. It would, however, be better that the Meeting should see the engine at work before he described it.

Mr. Shill lighted two gas jets in communication with the machine, and within two minutes the pressure was seen by the gauge to be 100lbs. to the inch. The engine was then started at about 800 revolutions per minute. It was stated that there was about half-a-pint of water in the boiler, and that the average consumption was about six pints per hour.

Mr. Moy said he would first make a few remarks on his visit to Vienna. At the Meeting of the Aëronautical Society there, the language spoken was German, which he was sorry he did not understand; but they had a large model balloon filled with gas, and fitted with an ingenious mode of stiffening. Mr. Ofenheim intended to use the same process in his elongated balloon; but he felt bound to tell him that his engine would never drive the balloon against the wind, and Mr. Ofenheim said he did not expect it would. He then told Mr. Ofenheim he would give him an engine which would not weigh more than 40lbs., and which would give the actual power of four horses. There was a small machine in the room at Vienna, which was driven round the room by the screw working in the air.

He must now say a few words about his own engine. By reference to a drawing behind him they would see how the water circulated. If they could see it actually they would be astonished at the rapidity with which the water circulated. It went at such a rapid rate that no sediment lodged in the tube. The water, passing rapidly over the heated metal, extracted all the heat from it, and kept the tube cool. By this means a large quantity of steam was generated. The engine had a stroke of two inches, and one-and-a-half inch piston, and it was using six-and-a-half times as much steam as they ever intended it to do, because they had removed the cut-off valve, and therefore they were actually wasting their steam; and it was now working at great disadvantage, through the furnaces and tubes not being enclosed. This engine was of very light weight. They could make them almost any weight, and for aërial purposes they could bring the weight down to 7lbs. per horse-power. The engine for Mr. Ofenheim would not occupy one-third of the space allotted to it, which was one square yard.

Captain Greenfield: Do you mean a cubic yard?

Mr. Moy: A *square* yard. As to height we do not want much. We can work at any reasonable pressure, and these tubes are practically inexplosive. The engines which we have made have been tested up to 500lbs. on the square inch, and they do not require to be worked over 200. We have not much body to make, and therefore can make it of great strength. It is very economical in working, because all the heat that gets in is stored up. There are no long passages to go through, and when it gets into the cylinder there is nothing to make the steam condense until its work is thoroughly done and all the power is got out of it. The circulation is perfect. Charles Wye Williams had more than twenty years ago said that whenever engineers designed boilers capable of rapid circulation they would obtain far better results than they did at that time.

The Chairman remarked that Mr. Williams had said it in that very room.

Mr. Moy went on to say that of course when people saw new things there were plenty of objectors. He would take two or three classes. One class of objectors said the tubes would stop up, as if the inventor was not likely himself to have prepared for such a difficulty, and be likely to see it first. The objector fancied that the inventor never saw anything of that. Now it so happened that the tubes did not stop up. Objectors said they would stop up; he had only to say in reply they did not stop up. These tubes had now been working more than twelve months. They had been working with common water, though in practice they meant to use condensed water. Yet the tubes did not stop up. Another objector said the circulation was so rapid that the tubes would wear out by circulation, so he expected these tubes would get as thin as a piece of paper in a short time; however, they did not. So he thought between these two classes of objectors they had got to the right point. There

was another class—the *cæteris paribus* people—of whom he would say more presently. Now he had given a hint that they were making an aërial machine. It was a working model, and would be 13ft. by 10ft., and 6ft. odd in height. It would have an effective lifting surface of 60 square feet always acting on the air. There would be a 4-horse power engine in it. Some people said this power was not wanted. He would tell them what he thought about that. When a boy wanted to learn to swim he began with bladders and continued until he could swim without them. So it was with power in an aërial machine. When they were coming down power was wanted, and of course power was especially wanted when they were going up. When the machine got in motion they did not want much power, and when his engine was at work in an aërial machine he should reduce the power by cutting off the steam earlier in the stroke until about one-tenth only of the power was used in rapid motion. What they wanted was power in starting to obtain speed; and power in coming down to control the descent. He then referred to the highly interesting experiments made by this Society which were reported upon last year. These experiments he had analysed, and from the data thus obtained he had made a geometrical table, whereby the lifting power and resistance of aëroplanes at angles from 90° to 5°, and at speeds varying from 10 miles an hour to 40 could be calculated. He should be happy to give further explanation to any one who desired information, and that was all he had to say that night, except in answer to any remarks that might be made.

The CHAIRMAN said this invention was an important one, and afforded a good illustration of the remarks he had made in opening the Meeting. The engine and boiler were not mere models, but were actually working. The engine was one possessing great power. He should be glad to know

whether on the same principle Mr. Moy could make a large engine.

Mr. Moy: A large engine would be proportionately lighter. A 100-horse power engine would not be more than 700lbs. weight.

The Chairman remarked that this engine had been found out through researches for aërial navigation, and from experiments which had been made from time to time until success was attained.

Mr. Shill said the engine had been at work twelve months and nothing had been done to it.

The Chairman: Did you use a condenser?

Mr. Shill: No; we feed it with ordinary water.

Mr. Moy: With a good condenser the consumption of water would be very much less.

Mr. Clare: We have not heard anything as to fuel. Can you give us the fuel per horse power per hour?

Mr. Moy: Mr. Burgh estimates 1lb. of coals per horse power per hour.

Mr. Clare: But it has only been worked with gas?

Mr. Moy: We can work with gas, petroleum, or anything you like. Mr. Ofenheim is going to use gas.

Mr. Clare: But nothing has been used except gas.

Mr. Moy: No.

A Member: I presume in practice this flywheel would not be used.

Mr. Shill: No.

The Chairman: I must ask you to give your warmest thanks to Mr. Moy and Mr. Shill. That they should have produced an engine which will help us to descend gently is a very great thing indeed. I am sure, in giving those thanks, we all wish the aerial machine may be successful. It was plain that the Austrian Society, by spending £1,200. on a balloon, considered the balloon was essential to aërial navi-

gation. There was no balloon belonging to this Society, so that they were really depending upon experiment. It was very likely they would have to proceed step by step, and to use the balloon as a raising power, and dispense with it by degrees. With Mr. Moy this result had been accomplished little by little. So should the Society reduce the use of the balloon till they could do without it. It was just possible if they had a balloon of their own they would be independent of caprice, whim, and chicane; for, so far as his experience went, no aëronaut he knew wished to improve the balloon. That was not the object of the Society. They wanted to improve, and to do away with the balloon entirely. The balloon was a tyrant which took you where it would. Sometimes you came down very agreeably, and sometimes under very unpleasant circumstances. He felt more struck with that Meeting than any other they had held before, and he did hope that when they met again they would be able to say they had progressed still more. He would now close this Meeting, but he was sure Mr. Moy and Mr. Shill would be willing to give any explanation; and he was convinced they were all much obliged to them for what they were doing in this direction. (Cheers.)

The Meeting then separated.

The following Paper communicated by a Member of the Society could not, from its great length and the number of diagrams necessary to illustrate it, be read at the General Meeting.

WINGS FOR MAN.

BY

JAMES ARMOUR, C.E.

PREFACE.

In the short work here prefaced, the Author seeks to determine approximately the sustaining power of planes disposed in such manner round the axis of a wheel that, when the wheel is put into rolling motion upon the ground, the increasing velocity of rotation may give air pressure to the planes, to float the weight and propel it on a forward course when floated.

The manner in which the question is treated will show that the title "Wings for Man" signifies not wings absolute, but wings in the form of a proposition simply.

JAMES ARMOUR.

Gateshead,

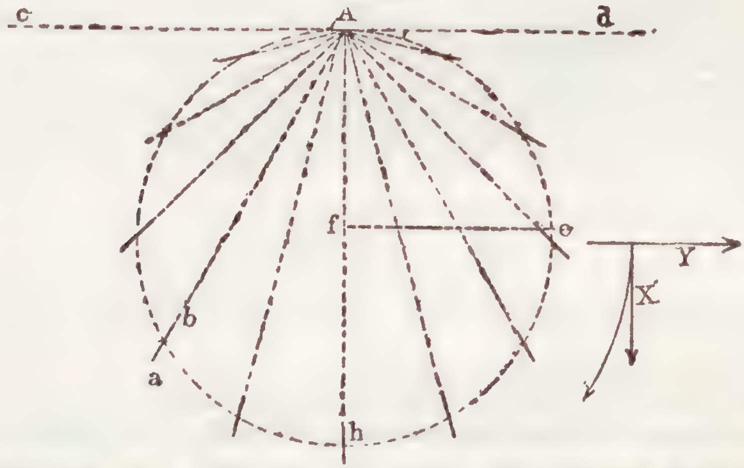
August, 1873.

WINGS FOR MAN.

CHAPTER I.

IN Fig. 1 we have 12 planes set round the circumference of a wheel of 8ft. diameter, and relatively inclined as shown, so as all to radiate from the topmost point A.

Fig. 1.



We assume these planes to have long length parallel to the axis of the wheel, and to be looked at endwise in the figure, so that their breadth alone is shown; and that breadth is assumed to be 1ft. $= ab$, to make the values of the sines and cosines of the angles as they stand in the Tables represent the area of air displacement in simple relation to the actual breadth 1·0; and will first treat them as if each measured only $1 \times 1 = 1$ square foot.

Taking the angles with reference to the horizontal line cd, the areas of air displacement earthward in the direction X will be represented by the cosines, and the areas in the direction Y by the sines.

(2) If the wheel, with no motion round its axis, and with the planes disposed at the angles shown, were free to fall in the direction X, the area upon which the resistance to air displacement took place, would be to 12 square feet as the sum of all the cosines, divided by 12 for the number of angles employed, is to 1 square foot, or nearly as 0·6366 to

1·0; which gives $0·6366 \times 12 = 7·6392$ square feet projected or cosine area of displacement.

Were the wheel, with its planes thus stationary, moved bodily in the horizontal direction *Y*, the projected or sine area of displacement would be similarly 7·6392 square feet; as in both cases, however, the rearward planes are variously screened from the air pressure by the planes in front, the actual resistance would be represented by a less area uniformly open to the pressure.

It is not necessary here, however, to determine the loss due to the screening of the planes in the rear, as this loss has reference not to the angles of inclination, but simply to the position in the wheel.

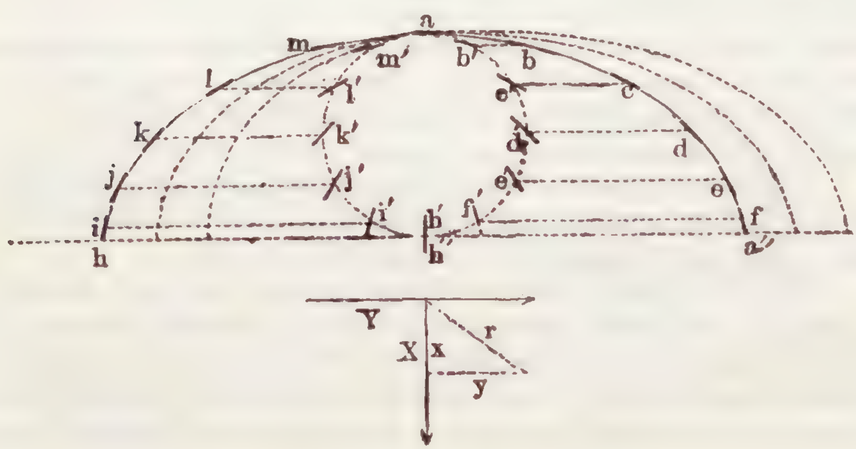
(3) Supposing the wheel, without either *Y* or *X* motion, were to revolve round its axis, we would have the plane *A* describing an angle of 90° in its passage from *A* to *h* on the arc *Aeh*; that is, would have it changing its value as an area of resistance from 0·0 at *A* to 1·0 at *h*; and we get the ratio of resistance parallel to *fe*, to that parallel to *Ah*, by dividing the diameter by the half circumference; thus, with the diameter equal 1·0,

$$\frac{3·1416}{2} = 1·5708 = Aeh,$$

$\frac{Ah}{Aeh} = \frac{1·0}{1·5708} = 0·6366$ for *X* to 1 for *Y*, mean value on the arc, when the value *Y* at *h* is 1·0 (*pars.* 14, 32).

(4) If, while the wheel is thus turning on its axis, we move it bodily in the direction *Y*, the plane, in the act of making one half-turn *ad'h'* round its axis, actually describes the curve *aa''* of Fig. 2, and this without

Fig. 2.



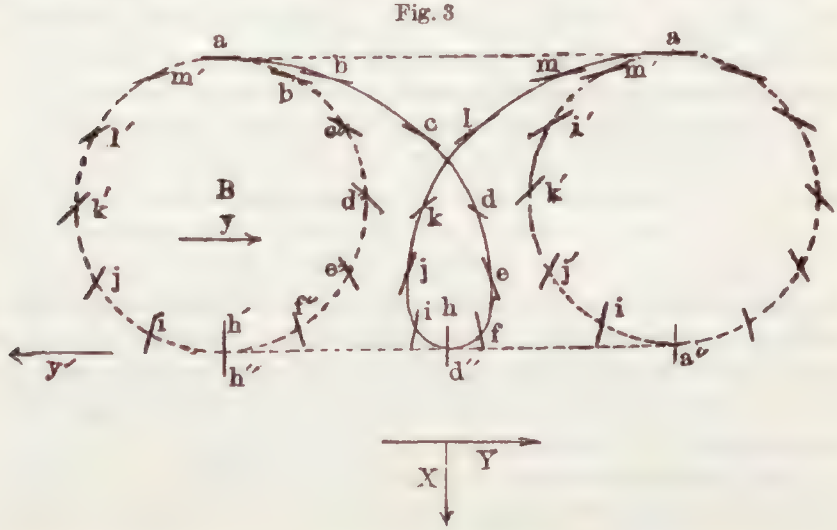
altering the 0·6366 mean area value of the plane, considered apart from the greater bed of air support; because, on the curve *aa*″, the change in the angle of inclination from 0° at *a*, to 90° at *a*″, is identical with the change from 0° at *a* to 90° at *h*′, when there is no *Y* motion; and the *Y* motion of the whole wheel merely carries the plane forward over a greater surface of air, by the resistance to displacement of which the weight of the wheel has to be floated.

(5) If the *Y* velocity be equal to the forward velocity the wheel would have if running upon a rail, the line of the curve will be parallel to the inclination of the planes, at the several points lettered on the cycloidal curve *aa*″, and will be the line of the resultant direction due to the mutual deflexion of the motions *Y* and *X* at these points, in the same way as *r* (Fig. 2) is the resultant direction when *Y* and *X* are represented proportionately by *y* and *x*; consequently, if the wheel be really running upon a rail, it will remain upon the rail till the *X* pressure developed by increasing velocity of rotation becomes greater than the weight.

(6) Suppose the wheel free in air, and revolving on its axis at a rate that, if running upon a rail, would carry it from *h*″ to *a*″, in making one half-turn; but, in the absence of a rail, we will assume that the velocity is sufficient to carry it against the resistance only the distance *h*″*d*″ (Fig. 3.) in the time of one half-turn

The curve which the planes will actually describe in this case is shown in Fig. 3; and as the planes in describing the curve have, at the

Fig. 3



successive points lettered a to m in the curve, the respective angles of inclination shown at the corresponding successive points lettered a to m' in the wheel, it is seen that the planes in Fig. 3 are driving the air before them, with an area of displacement at any point equal to the sine of the angle which they form with the curve line at that point.

Moreover, in the rising curve hm, the resistance which the air offers to displacement acts in depressing the wheel, and thereby neutralizes the upward resistance on the falling curve ah; consequently, as regards support to the weight of the wheel, the planes might as well assume simply the inclination of the curve, as in Fig. 2; and it is clear that this would render the lower portion of the curve of little service for the supporting of the weight, while the curve ah or hm so little exceeds the half circumference of the wheel that the surface of air over which the planes are carried, as regards the upper portion of the curve, exceeds only in like small proportion the surface that would be come in contact with, were the wheel revolving without Y motion, or with the axis stationary. It is apparent, however, that the planes in the rising as well as in the falling curve of Fig. 3, contribute to the motion Y.

(7) Supposing the wheel were suspended by long rods attached to the two ends of the axle, these rods would naturally hang plumb when the wheel was at rest; but were the wheel put in motion round its axis, in direction from A to e and h (Fig. 1), the inclination of the planes would produce air resistance tending to move the wheel bodily in the direction Y, and the suspension rods would be inclined from their original plumb direction; and if the length of rod were taken as the radius 1·0, and made to represent the weight of the wheel 1·0, the sine of the vertical angle formed by the line of inclination of the rod would represent the proportion which the air pressure bore to the weight 1·0.

In this case, with a given air pressure produced by a given velocity of the planes round the axis, the suspended wheel would come to balanced rest as regards Y motion when the angle was reached that gave a sine bearing the same proportion to the tabular radius that the pressure bore to the weight of the wheel; and as the air pressure has here lifted the weight to a height indicated by the versine of the angle, and expends its force in sustaining the weight at that height, it is clear that the force would impel the weight forward in the Y direction were the suspension rods removed, and the axle ends supported upon blocks free to slide in the direction Y.

(8) The friction of the supporting blocks sliding upon rails well lubricated would be for iron surfaces ·07 of the weight in motion;

whereas, if the sliding takes place upon air, the planes in this case acting as the sliding blocks, we have, in the first term K of Morin's Rule for air resistance, the co-efficient which is of the nature of friction, as it is independent of velocity, and likewise of weight of body, and is simply proportional to the surface in contact with the air; the atmospheric pressure taking the place of the pressure of the weight of body which is in action in ordinary friction between solids.

$$KA + K^1 V^2 = R \text{ in lbs.}$$

When the area A and the velocity V are expressed in feet, the rule is

$$A\left(\frac{K}{0{\cdot}00737} + \frac{K^1}{0{\cdot}0016} V^2\right) = R \text{ in lbs.}$$

So that, for an area of 1 square foot, the resistance due to air friction is 0·00737lbs., without reference to the velocity, or to the pressure which represents weight, due to the velocity. This, however, concerns merely the sliding friction, due most likely to the rolling motion imparted to the air in close contact with the surface.

(9) When the wheel moves with Y velocity, the rotating planes describe curves, which open out wider and wider from the rim of the wheel, as the Y motion, starting from a state of rest, increases until the Y motion becomes equal to the motion of rotation round the axis.

In Fig. 3 we assume that the rotation of the wheel would carry it from h'' to a'' in the time of one half-turn, if running upon a rail, but that the resistance to Y motion is so great that the pressure of the planes can impel the air-borne wheel only the distance $h''d''$.

If the centre of the wheel were retained at B (Fig. 3), the force in the rotating vanes would be expended in putting the air in motion in the direction Y^1; but, suppose the wheel free to move in the distance $h''d''$ in the time taken by the plane a to descend to h'; then, let $F = f + f^1$ represent the whole force; $M = y + y^1$ the motion of rotation; f the resistance of the air to displacement under the pressure of the rotating planes; f^1 the force lost in the motion y^1 given to the portion of air displaced; we have

$$MF - fy = f^1y^1.$$

It is clear therefore that when, as in Fig. 2, the velocity y is equal to the velocity of rotation, the backward loss f' has come to an end, while the force f has reached its maximum, that is, the force F of the planes is now expended wholly on the Y path.

(10) If the wheel in Fig. 2 had only the motion of rotation round its axis, the resistance of the plane a in its descent to h' would at any point be upon an area of displacement equal to the sine of the angle

formed at that point by the plane with the circular path it moved in; and the mean area represented by the mean sine, multiplied by the number of planes, would give the total area of displacement which the force F of the wheel would have to impel against the air resistance; and this total area is represented by A in the rule. $A\ (K + K^1 V^2)$.

(11) The definite values given in *par.* 8 to the coefficients K and K^1 are, however, for isolated planes on a straight course. In the case of a wheel with planes arranged radially from the axis, as in a common fan-wheel, the coefficients determined by Morin are

$$A\ (0{\cdot}00892 + 0{\cdot}001907\ V^2) = R \text{ in lbs.},$$

but, as the planes in the wheels now in question are not arranged radially from the axis, the coefficients precisely applicable would have to be specially determined for the particular form. As, however, the motion Y spreads the planes as shown in the curve of Fig. 2, we have to treat the forces with reference to this curve.

The motion of displacement is assumed to be in the planes and not in the air; the mean angle of inclination of the curve is about 57½°, formed with the direction of the displacing pressure X; and as at this angle Thibault found that the ratio of resistance to projected area of displacement, is the same as when the plane surface is perpendicular to the direction of the pressure, we employ the coefficients of *par.* 8.

In Thibault's experiments the resistances determined the higher coefficient values given in this paragraph; but he rotated the planes round a fixed axis, whereas here the planes are spread out upon the curve haa''; and in the absence of data for the precise value on the curve, we employ the lower coefficients for isolated planes on a straight path, because the conditions seem not to justify the employment of the higher values.

(12) In the rising curve ha, as in aa'', the plane moves in the resultant line of the two motions Y and X, but in the contrary direction in ha to that given to it in aa''.

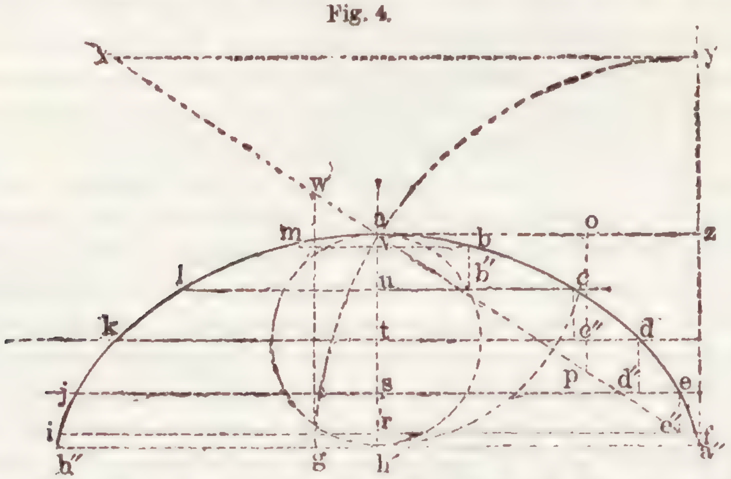
The velocity Y being here equal to the velocity of rotation, there is no backward force f', so that the rising plane can contribute nothing to the Y motion; but in its rapid flight over the extended surface of air at rest, ha, it contributes to the support of the weight of the wheel; the inertia of the bed of air passed over in the whole curve haa'' forming the resistance by which alone the weight can be supported; and, by reason of this inertia of the bed of air between a and a'', are the planes enabled to draw the wheel forward with the velocity Y, as each successive plane describes its path on a curve in advance of the curves of the preceding planes, as shown in the dotted curves of Fig. 2.

(13) The plane which for the moment happens to be at the foot of the curve a'', might be thought to act in the same way as the friction of a wheel upon a rail, the horizontal direction of the inclination of the planes on the upper part of the curve enabling the vertical plane at a'' to serve possibly as a fulcrum; but there is no motion in the plane there at a'' to develop air resistance, and the pressure of the planes upon the curved air path aa'' takes the place of the frictional hold of a wheel rolling along the ground.

Chapter II.

(14) In Fig. 4 let $h''a''$ represent the motion Y, and ah'' the motion X,

Fig. 4.



in the time of one half-turn of the wheel; aa'' will then represent the resultant motion, and the angle $aa''h''$ will be about 32° 29′, the sine ah of which is 0·53705, and the cosine $h''a''$ 0·84354. The cosine here is the sine of the complementary angle $h''aa''$; and with reference to the sines, as we shall presently show, the angle $h''aa''$ relates to X pressure, and the angle $aa''h''$ to Y pressure. The respective sines of these angles are components of the whole force equal 1·0 of the plane, and represent the relative proportions in which this force is expended upon the two resistances X and Y; and

$$\frac{Y = 0{\cdot}53705}{X = 0{\cdot}84354} = 0{\cdot}6366 \text{ for } Y, \text{ to } 1 \text{ for } X = \text{the tangent } op \text{ (see par. 33).}$$

This simply represents the proportion which ah'' bears to $h''a''$; and as the whole force is 1·0, and the component forces together cannot be more than that, we have

$$\begin{aligned} ah'' &= 0{\cdot}53705^2 = 0{\cdot}2884 \\ h''a'' &= 0{\cdot}84354^2 = \underline{0{\cdot}7116} \\ & \qquad\qquad\quad 1{\cdot}0000 \end{aligned}$$

The spaces between the successive points a, b, c, d, e, f, a'', in the curve, though unequal, are travelled in equal times.

It is apparent from the difference in these curve spaces that, at a, the velocity in the direction Y is at its maximum, and that, at a'' it has sunk to zero; and it is seen that this difference in the curve spaces is owing to the variable rate of the motion of the planes in the path of rotation round the axis.

(15) When ah'' is 8ft., and the wheel with its twelve planes makes one revolution per second, we have, approximately,

	ft.		sec.		vel.	
av =	0.53	÷	$\frac{1}{12}$	=	6·36	feet rate per second.
vu =	1·47	÷	,,	=	17·64	,, ,,
ut =	2·00	÷	,,	=	24·00	,, ,,
ts =	2·00	÷	,,	=	24·00	,, ,,
sr =	1.47	÷	,,	=	17·64	,, ,,
rh =	0·53	÷	,,	=	6·36	,, ,,
	8·00				95·90	

Were the resistance to motion to vary simply with the velocity of motion, we would have 95·90 ÷ 6 times for aa'' = 15·983ft. mean velocity for ah'' in the descent on the resultant aa''; and as the wheel makes one revolution in one second, the velocity of the planes on the path of rotation round the axis is at the rate of 25·13ft. per second, and

$$\frac{15{\cdot}983}{25{\cdot}13} = 0{\cdot}636\,;$$

but as the resistance varies with V^2 we square the rates of velocity given above, find the sum to be 1855·238, and

$$\frac{\sqrt{1855\cdot 238}}{6 \text{ times}} = 17{\cdot}58\text{ft. mean } X \text{ velocity force.}$$

At 15·983ft. uniform mean velocity the plane will descend from a to a'', and the resistance impelling it in the Y direction will be as the sine ah'' = 0·53705, while the vertical resistance opposing the motion X will be as the cosine $h''a''$ = 0·84354; apa'' in relation to sine and cosine representing the actual area = 1·0 upon which these relative resistances are developed (*par.* 34).

It is clear, however, that to find space for the 15·983ft. in the given half-second of time for apa'', which measures only about 14·9ft., the plane has to move outward on the longer path of the curve ada'', and we find, approximately, that

$$14{\cdot}9 \ : \ 15{\cdot}983 \ :: \ apa'' \ : \ ada''.$$

(16) The motion X, or the tendency earthward, is acting as freely in the cycloidal curved path aa'', or in ah'' the path of rotation round the axis, as in a direct fall from a to h'', when the conditions of resistance give the same time to each in descending from a to the level of $h''a''$, so that we are here free to reason as if the descent were in vertical direction from a to h''.

Assuming that the plane descends in this vertical direction, the work in foot-pound units performed in the displacement of the air in the path of the plane, would, per second, be at the rate of $R = A\ (K + K^1 V^2) =$ compressive force, multiplied by the space in feet that the mean velocity would carry the plane in 1 second; while a column of still air, in transverse area equal to the area of displacement of the plane, and of height sufficient to contain weight to balance R, would represent the constant compressive force acting uniformly at all points of the time, and of the space travelled at the uniform mean velocity; so that the weight of the column of still air merely represents, in simpler form, the weight of force in the volume of air which is undergoing compression under the front face of the advancing plane.

(17) Now, at the beginning, near zero at the point a, this column, which by its simple weight merely represents the force R, would be very low in height; and any increase of acceleration in the velocity as the plane fell further would only increase the height of the representative column, to maintain the balance of column weight against the increased weight of V^2 pressure in the plane; and the force of inertia of the weight of pressure in the plane represented by the weight of air in the column, at any point of the development, will be $\frac{W}{g} \times \frac{v}{t}$, when W is the weight of the pressure at that point; g $32\frac{1}{6}$ for free standard gravity; v the velocity; and t the time for which the velocity is rated (*pars.* 21, 35).

The column by its weight represents the force of air resistance offered to the plane, that is, represents the pressure of the plane, and, as the plane with this pressure is in motion with velocity v rated for the time t, and further, as the pressure on the plane is equivalent to weight, and as the force of inertia here is the resistance which the weight opposes to increase of acceleration, we have the force of inertia in the balancing column equivalent to the force of inertia in the motive pressure of the plane.

As the plane is exerting its pressure in the actual displacement by compressive force of a weight of air, the force of inertia of which is

constant as regards the uniform power required to overcome its inertness when the rate of acceleration of the velocity is uniform, but is accumulative in respect to the weight's retention of the power expended on it; thus, we have, employing the velocity v for 1 second for free gravity, with a weight of 1lb., $\frac{v^2 W}{2g} = \frac{32\frac{1}{6}^2 \times 1\text{lb.}}{2 \times 32\frac{1}{6}} = 16\frac{1}{12}$ units of work accumulated or stored up in the 1lb. weight in motion, ready to be thrown out into sensible form upon any obstruction arresting the motion, but in its passive stored-up state remaining perfectly independent of the foot-pound work which the 1lb. weight would perform in moving through air, supposing the resistance of the air to render the velocity uniform from the point where the given $32\frac{1}{6}$ft. per second rate was reached.

The quantity of accumulated work thus stored up within the weight is determined by the rate of the velocity v at the point of observation, and is ever the same quantity for a given rate of v, irrespective of the time allowed for the accumulation, or for the development of v; it is clear, therefore, that when the plane with acquired X motion is shifted edgewise by the Y motion on to fresh air at rest, and thereby shifted from the body of air to which it has already imparted the given X velocity (which is v of the above equations), it requires, from the store of accumulated inertia work belonging to its own pressure weight, to expend force in overcoming the inertia of the fresh body of air; and as the accumulated force of inertia in the pressure weight of the plane was simply equal to the accumulated force in the body of air shifted from, the force of attraction between the earth and plane supplying the constant motive power in the plane to continue the work, it is evident that the new body of air shifted on to will require all the force stored in the plane to give it motion equal to the motion in the preceding body, and this will be equivalent to a fresh start for the plane, and there will consequently be as many fresh starts as there are distinct shifts on to fresh air in the Y space travelled.

The force of inertia developed by accumulation in the time required to make one shift may then be termed the unit of force, which, multiplied by the number of shifts in the time of the given Y velocity, gives the air inertia resistance R' for one second; so that as the weight of the R columns of still air, which represent the compressive force due to v^2 of the plane, is of equal value to the weight of pressure in the plane, power in the plane is required to balance as many R columns as there are distinct shifts on to fresh air in the Y space travelled.

(18) The mean velocity is 15·983ft., but as the plane in travelling

the curve experiences resistance due to the square of the successive velocities that give this simple mean, we employ the square mean given in *par.* 15.

When the area of displacement is perpendicular to the direction of the motion, the X resistance to the plane at the mean-square uniform velocity of 17·58ft. per second, is

$$\begin{matrix}\text{sq. ft.} \\ 1{\cdot}0\end{matrix}\left(\begin{matrix}K & & K^1 \\ 0{\cdot}00737 & + & 0{\cdot}0016 \times 17{\cdot}58^2\end{matrix}\right) = 0{\cdot}502\text{lb. air pressure.}$$

(19) But, as the mean area of X displacement is only 0·8435 of the value 1·0 for the full area displacement were the plane constantly perpendicular to the direction of the path it moved in, we have the mean air resistance only $0{\cdot}502 \times 0{\cdot}8435 = 0{\cdot}4234$lb., which we employ as the mean constant force for each square foot of plane at the velocity named; and as we have thus reduced the force of the air pressure or resistance to the mean constant value for the whole curve which represents one turn of the wheel round its axis, we are free to employ 0·4234lb. as the weight of the mean column of air which would balance the mean pressure; and as the mean breadth of the plane in the direction Y is assumed to be 0·8435ft., we have this unit area making $\frac{25{\cdot}13\text{ft.}}{0{\cdot}8435} = 29{\cdot}792$ shifts of 0·8435ft. each, on the Y space of the whole curve haa'', so that there are 29·792 distinct columns of air of the mean 0·8435 value requiring the force of inertia due to the given velocity to be developed in them, by the pressure of the given X velocity, in place of one column only, were the plane, starting from a, to move on a straight path perpendicular to its area of displacement. The X velocity is 17·58ft. for one second of time, and one second is allowed for the plane to act upon the inertia of the whole mass of air, which has its area of resistance on the curve haa'', and which we say is equal to 29·792 mean columns of inertia resistance; so that we have the 0·8435ft. inertia resistance (*par.* 20) multiplied 29·792 times on the curve, and the multiple quantity is equal to the weight which the plane area, moving at the given rate upon the curve, will sustain in air (*pars.* 20, 22).

(20) As the time assumed for one turn of the wheel, and therefore for one complete curve, is one second, and as there are in effect 29·792 successive columns of air to resist the pressure of the plane by the inertia of their weight, we divide one second by 29·792, and likewise 15·983ft. velocity, to get the $\frac{v}{t}$ for the time of one clear shift of position of the plane on the curve haa''.

We employ the mean of the simple velocity here, in place of the mean V^2, because the result we now seek is ruled by the rate of acceleration, and in the mean simple velocity 15·983 we have the mean rate, then

$$\frac{1{\cdot}00 \text{ sec.}}{29{\cdot}792} = 0{\cdot}0335 \text{ for } t.$$

and

$$\frac{15{\cdot}983\text{ft.}}{29{\cdot}792} = 0{\cdot}532 \text{ for } v.$$

then, employing the 0·8435 mean value of the resistance, which is 0·4234lb., we have $\frac{W}{g} \times \frac{v}{t} = \frac{0{\cdot}4234\text{lb.}}{32{\cdot}18} \times \frac{0{\cdot}532}{0{\cdot}0355} = 0{\cdot}21$lb., constant value of the inertia of 0·4234lb. weight of air, at the given mean rate of acceleration from zero to 15·983ft. velocity in one second, and 0·21 × 29·792 shifts = 6·2563lbs. resistance of the air inertia in one second, on the whole curve *haa''*, for each square foot of plane.

(21) At the rate of acceleration due to natural gravity, with $g = 32\frac{1}{4}$ft. per second, the constant value of the pressure weight would be simply the weight 0·4234lb., because $32\frac{1}{4}$ for g represents in the form of motion the natural force of the earth's attraction, close to earth ; and this force in constant action upon matter gives simply the effect called weight, and in this case the equation would be

$$\frac{W}{g} \times \frac{v}{t} = \frac{0{\cdot}4234}{32{\cdot}18} \times \frac{32{\cdot}18}{1{\cdot}0} = 0{\cdot}4234.$$

(22) As the active force of the plane pressure is developed on the falling curve *aa''*, and as 0·4234 is the mean X pressure per square foot of plane area, and, further, as the X pressure area is in direct relation to $h''a''$, which measures 12·566ft., we determine the resistance by the pressure on the curved path *aa''*, and find it closely approximating to the inertia value for the whole path *haa''* ; it being borne in mind here that the inertia of air, as of anything possessing weight and come upon in a state of rest, concerns displacing force simply, irrespective of the direction that the force is moving in.

The rising planes on the curve *ha*, though in motion contrary to the direction of the X pressure, are sustained in common with the planes on *aa''*, by the inertia of the elastic air bed that they travel on in rising, the assumption being that the force on the falling curve *aa''* is taking effect in displacement of the wheel and not of the air.

$0{\cdot}4234 \times \frac{29{\cdot}792}{2} = 6{\cdot}30$lbs. resistance for the path *aa''* ; but as the pressure which meets with this resistance is imparted by a plane surface,

inclined so that the air, to get relief from the imposed pressure, must either suffer itself to be displaced to the rear edge, or else support the pressure so as to let the plane slide; in the latter case, which we assume to be what happens under the conditions previously named, we have the plane displaced and not the air (*par.* 36). And as the inertia of the air is the only sustaining force, we have the displacement motion Y of the falling planes in aa'', keeping the rising planes pressed against the air they slide upon in ha, so that the inertia of the whole elastic bed of air is made to bear the X pressure of the gravity of the dead weight, and the faces of the rearward or rising planes are prevented from opposing the Y motion by rising with their angle of inclination coincident at all points with the angle of the curve they travel on.

(23) We will now assume that the weight to be supported by the plane is 6·3lbs., and, employing Morin's Rule in inverted manner, will find the velocity that will give air resistance to balance this weight, so as to float it on a balanced horizontal line: thus—

$\frac{6 \cdot 30 - 0 \cdot 00737}{0 \cdot 0016} = 3933 = V^2$, and $\sqrt{3933} = 62 \cdot 71 = V$; then $\frac{\cdot 3933}{2\,g} = \frac{3933}{64 \cdot 38} = 61 \cdot 09$ft. space fallen to give 62·71ft. velocity; and $\frac{61 \cdot 09}{14 \cdot 896} = 4 \cdot 1011$ft. mean fall in the time taken for 1 square foot area of plane to move $\frac{1}{14 \cdot 896}^{\text{th}}$ part of the distance $h''a''$; then

$4 \cdot 1011 \times 64 \cdot 38 = 264 \cdot 02 = V^2$, for 4·1011ft. fall; and

$\sqrt{264 \cdot 02} = 16 \cdot 24$ft. $= V$, for 4·1011ft. fall,

$K^1 \times V^2 = 0 \cdot 0016 \times 264 \cdot 02 = 0 \cdot 423$lb. constant X resistance per square foot of plane perpendicular to X, for the V^2 due to the time of one clear shift in the direction Y; and $0 \cdot 423 \times 14 \cdot 896$ shifts $= 6 \cdot 3$lbs. resistance for X in the whole Y distance $h''a''$.

(24) Now in this we have the square foot area of plane perpendicular to X at the 16·24ft. rate of velocity, whereas the plane changes its angle of inclination from 90° at a to 0° at a'', in falling along the curve aa'', thereby giving a mean area of 0·8435 square feet only; and as the value of velocity force is as V^2, we have—

V^2		V^2		Area.		Area.
$17 \cdot 58^2$	:	$16 \cdot 24^2$	: :	1·0	:	0·8435 nearly.
309·056	:	264·02	: :	1·0	:	0·8435 ,,

The ratio would be found strictly as 1 to ·8435, were the mean-force velocity 17·58 on the falling curve determined with greater precision than by the mean simply of six spaces, as in *par.* 15.

(25) A heavy body starting from a state of rest in space near earth will have only the natural force of gravity, with the velocity accelerating at the natural rate of 32⅛ft. per second; and the 62·71ft. velocity due to a fall of 61·09ft. will require for its development $\frac{62{\cdot}71}{32\frac{1}{8}} = 1{\cdot}94$ seconds; and if it weighs 6·3lbs., and has an area of 1 square foot, and is moving in a straight course to earth, perpendicular to the area of resistance, the velocity will become uniform at the velocity named.

In the wheel the plane with X motion starts from zero, corresponding to a state of rest, at a; but the Y motion brings it into contact with the displacement area of a body of air 14·896 times the displacement mean area for a straight course earthward, and we have the force of $62{\cdot}71^2 \times 1{\cdot}0$ equal to the force of $16{\cdot}24^2 \times 14{\cdot}896$; the greater volume of air with its low development of inertia force here taking the place of the less volume with its higher development (*par.* 17); and we have already seen that, owing to the change in the angle of inclination of the plane, the mean force of nearly $17{\cdot}58^2 \times 0{\cdot}8435$ is equivalent to $16{\cdot}24^2 \times 1{\cdot}0$.

(26) The centre of gravity of the weight is in the axis of the wheel, and as the motion by which the planes develop sustaining resistance equal to the weight is on a circular path round that centre, we have the Y component of the plane pressure propelling the sustained centre in the Y direction (*par.* 44).

(27) We have yet, however, to determine the value of the planes on the curve in relation to horizontal or Y motion.

The 0·8435 value of the force exerted by the planes has reference merely to the resistance opposed to gravity, the pressure producing the resistance being exerted in the purely vertical direction X, and the resistance simply balances the gravity of the load.

(28) When the wheel (as in Fig. 1) has only the velocity of rotation round its axis, the pressure of the plane A in starting downward begins at zero, and becomes equal to 1 only on reaching the bottom position at h; the rim of the wheel in this case carrying the points A and h along the path of rotation with equal velocity.

(29) In Fig. 2 the extension of the path on the curve enlarges the air surface on which the plane a has to act in a given space of time, and thereby in effect enlarges the supporting area, or the area of resistance to the force of gravity, the extension of the path upon the curve giving the greater inertia of a greater weight of air to oppose the force acting in the plane.

The enlargement of area, however, has taken place by the

horizontal extension of the path, while the vertical space ah'' remains constant, and with it likewise remaining constant, the mean simple velocity 15·983ft. for X; so that, as the successive times marked a, b, c, d, e, f, a'', on the curve, are equal, and coincident with the times of rotation a, b', c', d', e', f, h', on the rim of the wheel; and further, as the angles of inclination of the plane at the successive points on the curve thus lettered are precisely the angles at the coincident points on the wheel rim, we have the area of pressure upon which the resistance X is developed on the curve in Fig. 2 equivalent to the area upon which the pressure X is developed in Fig. 1; because the extension of the path consists simply of the flattening out, in the direction Y, of the wheel-rim $ad'h'$ which carries the planes, the planes assuming the successive angles without reference to this; so that the plane A of Fig. 1, represented by a of Fig. 2, is carried forward in the Y direction the distance $h''a''$, in addition to the X space ah'' which it would descend if merely rotating round the axis of the wheel, as in Fig. 1.

(30) Then, as regards air resistance due to motion, zero being at the point A in Fig. 1, and at the point a'' in Fig. 2, the plane in Fig. 1 has its maximum force pressing in a way to produce Y motion, and in Fig. 2 has it developing X resistance; hence, when the pressure Y on the arc Aeh of Fig. 1 is represented by 1·0, the angle which gives the 0·6366 ratio tangent force for X (*pars.* 3, 14) opens from h with A as centre; that is, the direction of the X component force is parallel to Ah.

(31) Whereas, when the maximum developed resistance X, on the curve ada'' of Fig. 2, is represented by 1·0, the mean ratio angle may be taken as opening from h'', with the zero point a'' as centre; and we then have the 0·6366 tangent ratio for Y, that is, the direction of the Y component force in Fig. 2 is parallel to $h''a''$. We are free to take the angle in Fig. 2 as opening from h'' with a as centre, and, in that case, the Y ratio will be represented by the cotangent of the angle which has a'' for centre.

CHAPTER IV.

(32) In explanation of the mean ratio we may here observe that, referring to Fig. 4 and employing $a''a$ as the radius 1·0 representing the whole force, equal 1·0, the length of the complementary arc ya, belonging to the complementary angle $aa''y = 57·2957°$, is equal to the length of the radius; and dividing this number of degrees in ya by the 90° of yg, we have

$$\frac{57·295°}{90°} = 0·6366 \text{ ratio.}$$

Further, the complementary arc ay is in the same ratio to the arc ag that the cosine az is in to the sine ah'' of the angle $aa''h''$; and as $h''a''$ is equal to az, we employ the tabular values and get the ratio

$$\frac{ah''}{h''a''} = wg, \text{ as in } par. \ 14.$$

Moreover, we have the whole-force radius $a''a$ represented by $a''y$ in the same ratio to the cotangent yx, as ah'' is in to $h''a''$; thus—

$$\frac{a''y}{yx} = wg = \frac{1 \cdot 000}{1 \cdot 57} = 0 \cdot 6366.$$

And yet again—

$$\text{Rad.} \times \text{tang.} = a''g \times wg = 1 \cdot 0 \times 0 \cdot 6366 = 0.6366.$$

Consequently we have the cosine $h''a''$ relatively representing the radius for the whole force 1·0, and the sine ah'' the relatively proportionate part of this whole force which, in the turning of the angle of inclination of the plane from 90° at a to 0° at a'' is developing Y pressure.

(33) The sines, tangents, &c., employed in Fig. 4, simply exhibit in graphic form the relative proportions of the respective forces or motions, and are in nowise dependent for their value upon their individual lengths or the individual spaces enclosed by them, the value being ruled solely by the angle; thus, in *par.* 14, having made ah'' the radius, we have for the angle $aa''h''$ (equal to the angle zaa'') the tangent value represented equally truly by op; and we would have the tangent represented equally by a still shorter line were we to take as the tabular radius the actual radius $a\,t$ of the wheel.

(34) In the paragraphs preceding this, we make the resultant line of force aa'' represent the tabular radius for the whole force in order to exhibit in direct form in relation to it in $a''g$, the proportion represented by wg, which is developing resistance to give Y motion.

Now, as the whole force in the radius aa'' (Fig. 4) is 1·0, and as $h''a''$ is the component developing X resistance, we have $h''a = 0 \cdot 6366$ of $h''a''$, developing Y pressure; and as (*pars.* 19, 32) the whole force 1·0 is represented by the constant pressure $R = 0.4234$lb. air pressure per square foot of plane, we have for Y force

$$0 \cdot 4234 \times 0 \cdot 6366 = 0 \cdot 2695\text{lb. pressure } Y.$$

The ah'' ratio 0·6366 is in relation to 1·0 for $h''a''$; but as $h''a''$ is only 0·8435 of the actual area of the plane represented by $a''a$, it is clear that the 0·6366 has reference to the 0·4234lb. resistance, which is 0·502lb. × 0·8435, as in *par.* 19; otherwise we would have

$$0 \cdot 502\text{lb.} \times \overset{\text{sine.}}{0 \cdot 53705} = 0 \cdot 2965\text{lb. pressure } Y.$$

(35) The Y velocity is assumed to be at the rate of 25·13ft. per second, but it is evident that the plane, at any point, can exert pressure to give Y motion only in the ratio of its area of Y displacement perpendicular to X at that point, with the force there due to the velocity X.

Treating the Y pressure as we treated the X pressure in *par.* 20, to get the force of inertia of the body of air passed over, we have $\frac{W}{g} \times \frac{v}{t} = \frac{0 \cdot 2695}{32 \cdot 18} \times \frac{0 \cdot 532}{0 \cdot 0335} = 0 \cdot 13809$lb. constant value of the inertia of 0·2695lb. weight of air column resisting the given rate of acceleration; and as there are only 14·896 mean area shifts of the inclined plane on to new air at rest in the forward or falling curve *ada''*, we have $0 \cdot 13809 \times 14 \cdot 896 = 2 \cdot 0569$lbs. Y resistance of the inertia of the air upon the whole falling curve per foot of plane passing over it.

(36) As air, however, is an elastic fluid, and the formula here employed is for weight simply, without reference to elasticity, and as a certain measure of compression of the air in contact with the plane, and consequent yielding, must take place before the force of the compression equivalent to the given weight can be communicated to the air beneath, the force of inertia here determined forms merely a standard by which to determine the value of the pressure in the plane in motion; and, to keep the question in simple form, we do not in direct manner compute the extent to which the air may yield under the pressure, because the motion of the planes upon the curve is variable; and as this motion is the motion of displacement, we have it in the planes and not in the air, as soon as the point is reached where the extended air resistance balances the dead weight borne by the planes (*par.* 22).

(37) The actual pressure developing Y velocity is 0·2695lb, per square foot of plane, and as the velocity of rotation round the axis of the wheel that gives this pressure is assumed to carry the wheel forward in the Y direction at the rate of say 25ft. per second; and as the air resistance per square foot of perpendicular surface at this rate is 1·00737lbs., we have $\frac{1 \cdot 00737}{0 \cdot 2695} = 3 \cdot 738$ as the ratio of the required propelling area with its 17·58ft. mean V^2 power (*par.* 15) to 1·0 for the vertical area which will offer direct resistance to the forward motion Y only; that is, when the vertical front face area of the body is equal to $\frac{1}{3 \cdot 738}$th part of the mean propelling area, the power and the resistance will be balanced so as to make the Y motion uniform.

(38) The plane advancing constantly edgewise on the curve, has

the distance haa'' (Fig. 2) equal about 32ft., to travel in one turn of the wheel, here assumed to occupy one second of time; but as the air resistance varies with the square of the velocity, we square the successive varying rates of the velocities in the 12 spaces into which the curve is divided, and divide the sum by 12, and the square root of the quotient is, approximately, the mean velocity to which the actual resistance on the plane edge is due; thus, the sum of V^2 quantities is about 15092·8, and this divided by 12, is 1257·73, then

$\sqrt{1257 \cdot 73} = 35 \cdot 46$ft. $= V$ for mean edge resistance R on whole curve.

As, in the rise from h (Fig. 2), the edge resistance tends to depres the wheel in the direction X, while, in the fall toward a'', this X depression is balanced by the contrary tendency, we have the Y resistance R on the edge, in the same ratio to the whole edge resistance on the curve, as found for the face X resistance R, in *par.* 15, viz., 0·8435 to 1·0; so that the area of effective resistance for 1 square foot of edge is 0·8435; and

ft. area.
0·8435 $(0 \cdot 00737 + 0 \cdot 0016 \times 35 \cdot 46^2) = 1 \cdot 7036$lb. R for mean constant resistance to Y on the curve, per square foot of edge area.

(39) But the motive power will have to overcome the edge resistance at the full rate of 2·019lbs. per square foot of edge area perpendicular to the curve path; so that as we now assume that there are 180 lineal feet of edge in the planes, which have the velocity named, it is clear that the planes must be thin, or have their thickness tapered from the middle. We do not include this edge resistance in the 0·4234lb. resistance per square foot of plane area, and to give it a precise value per square foot of face area, would require the precise form and fashion of the plane to be here considered, and that lies outside of our present purpose; suppose, however, that the edge area for planes and stiffening-rods were equal in effect to only $\frac{1}{24}$th, or 0·0417 square feet, perpendicular to motion, per foot of plane; then $0 \cdot 0417 \times 2 \cdot 019$lbs. $= 0 \cdot 08419$lb. per foot of plane, and $0 \cdot 08419 + 0 \cdot 4234 = 0 \cdot 5075$lb. for motive power (*par.* 58).

Chapter V.

(40) We will now suppose the wheel resting on the ground, and started in motion from a state of rest, to acquire X pressure on the planes and Y velocity to float it. The weight to be floated by each square foot of plane is 6·30lbs. (*par.* 22), and the resistance R developed

for Y by the pressure of the plane, in the falling curve *ada''* is 0·2695lb. per square foot of plane (*par.* 34).

(41) The diameter of the wheel is greater than usually employed for wheels that run upon rough ground, and it will surmount the roughness with proportionately greater ease; but, with a given lightness of frame, greatness of diameter implies weakness.

(42) The Y power is $\frac{6{\cdot}30}{0{\cdot}2695} = 23{\cdot}38$ part of the weight to be carried; and as the Y power here acts in the manner of traction power, and as the ratio of 1 for traction power to 30 for load borne upon the axles of ordinary wheels, is common where the surface of the ground is otherwise than hard and smooth; and further, as the resistance to mere rolling on a surface not hard and smooth increases with the velocity, and the velocity proposed for the wheel before it leaves the ground is high; we will assume that, while on the ground, the Y traction developed in the falling curve is exerted only in overcoming the rolling and the axle resistance; and as all that the purely X pressure of the planes disposed as shown in the curve of Fig. 2 can do is to float the dead weight, so that when the X pressure and the weight are balanced, the Y pressure may propel it horizontally, it is clear that upon a level, with the planes inclined as in Figs. 1 and 2, the wheel, with the X pressure and the weight thus balanced merely, could not rise above the ground; though the pressure of the weight, transferred to the planes, would relieve the pressure on the ground, assuming the wheel strong enough to move on ordinary ground with sufficient velocity; and on reaching a sudden declivity in the surface of the ground, it would float out upon the air.

(43) If, however, the dead weight were only 6·0lbs., and the X pressure of the planes were 6·30lbs., this would be equivalent to an upward pressure force of 0·30lb. per foot of plane, and the wheel would rise with the force of that upward pressure, on a rising resultant. Moreover, if the planes were inclined as in Fig. 5, say by the canting of the platform inside, the rise would be more ready.

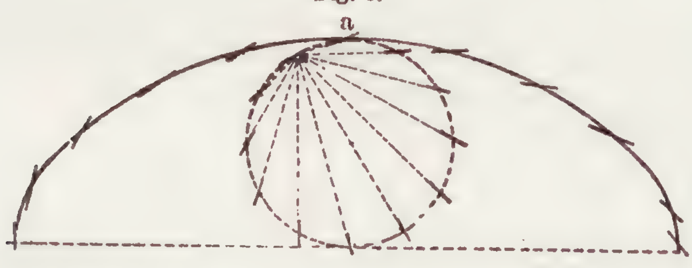
(44) Were there no velocity of rotation round the axis, but if, in place of it, the planes were spread out as shown in the curve of Fig. 2, and allowed to fall to earth to acquire the velocity that would give resistance equal to the gravity of the weight, in the fall to give this velocity, the rate of acceleration $\frac{v}{t}$ would gradually lessen, until the velocity became uniform at the point where the air pressure and the weight became balanced; but, to maintain this balancing pressure, the

series of planes would have to continue falling at this uniform velocity; whereas, when the planes are rotated round the axis of the wheel, we may regard the weight as centred in the axis, and the velocity that can be got only by falling, when there is no rotation, is here got by the velocity of rotation, with the axis and the weight centred thereat, relatively at rest, for the planes are revolving round it (*par.* 26).

(45) When the ground slopes at the angle of about 2° 14′, which gives a rate of inclination of about 1 for rise to 25·566 for slope, equal to 1 of fall for each turn of the wheel, we have this 1ft. of fall in addition to the mean velocity, 15·983ft. Roughly, this would make the mean-force velocity about 1 + 17·58 = 18·58, and this greater velocity substituted for the 17·58 in *par.* 18, will give a greater resistance for X in the curve, calculated to sustain a greater weight than 6·30lbs., or, equivalently, to carry 6·30lbs. weight horizontally forward clear of the ground.

(46) If, however, in order to raise 6·30lbs. from the level ground with the velocity due to one turn of the wheel in one second of time, the centre to which the angles of inclination of the planes converge be upon the rim in the rear of a in Fig. 5, so as to keep the planes always at the

Fig. 5.



angle of say 2° 14′ with the path of motion in the curve, we have the planes exerting a rising tendency somewhat similar to the tendency last referred to, to move away horizontally from ground that slopes at this angle. A certain measure of power, however, is lost to Y in the case of Fig. 5.

(47) Were the wheel going against the wind it would require less than a velocity of 25ft. per second in the wind to neutralize the Y impelling force of the plane on the falling curve, because the plane in the lower time spaces d, e, f, a'', is coming to a halt as regards Y motion, and, during the $\frac{1}{8}$th of a second, while travelling the spaces fa'', hi (Fig. 2),

is nearly at right angles to the direction of the motion of a contrary wind. For another ⅛th second in travelling the spaces *ef, ij,* the proportion of the whole Y space ha'' for 1 second which it travels, is at the rate of about 7·68ft. per second; and for another ⅛th second for the spaces *de, jk,* only at the rate of 18·60ft., the rate for the spaces *cd, kl,* being 31·56ft.; it is clear, therefore, that if an attempt were made with this particular form of wheel to rise from the ground, in the face of a breeze at the rate of 25ft. per second, or 17·4 miles per hour, it would be hopeless; indeed, there are few land-birds that can fly against a breeze of this strength; and those that may can make but slow Y progress even with the expenditure of more than usual power. A breeze acting upon their thin wings may help them to rise, but once off the ground they may be seen in ordinary circumstances to fly away on a line that forms a wide angle with the direction of the wind.

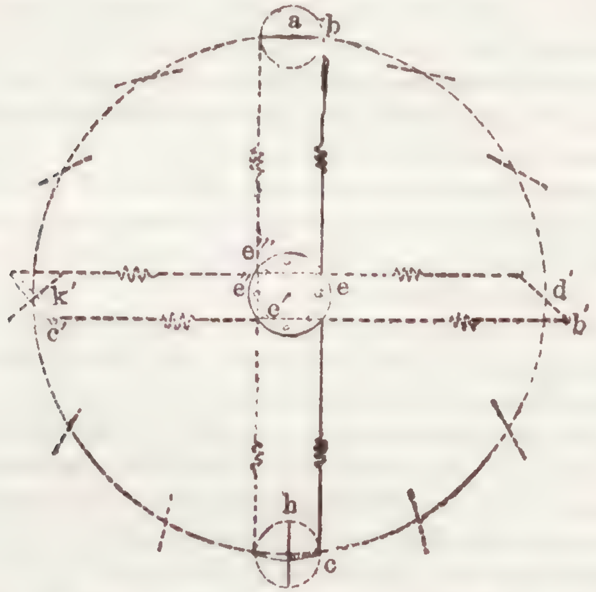
(48) A plane, launched from a to descend on the straight resultant apa'' of Fig. 4, with power to give it a Y velocity of 25ft. per second, measured in line with $h''a''$, would—neglecting edge resistance—have its propelling power balanced, or neutralized, by a horizontal current of wind of equal velocity to this acting directly against it, in line with $a''h''$, and the Y displacement area would be consequently powerless to produce motion in the Y direction, because the air in contact with the lower face would not wait to bear the pressure; and the wind in arresting the Y motion would have the same force on the projected Y area of the upper face that the corresponding area of the lower face would have were the wind to give up its motion to the plane; and as the air beneath is receding from the pressure, the weight of the plane is unsupported, the plane falls, and is at the same time blown backward.

Greater velocity on the path apa'' would bring the plane down X-ward upon the receding air at a quicker rate, so as to derive support, but this would require force greatly in excess of the power available in a wheel such as we have under consideration.

CHAPTER VI.

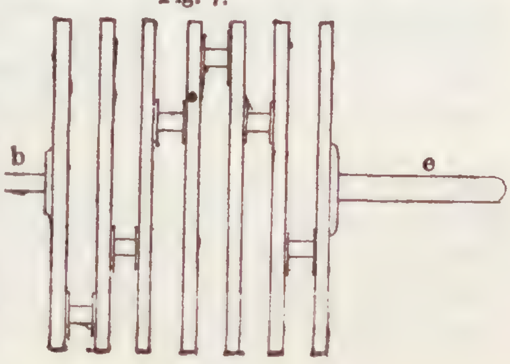
(49) We have now to consider the means by which the planes may be made to take the angles of inclination shown in Fig. 2, and employ Figs. 6 and 7 to exhibit them.

Fig. 6.



As the angle changes 90° in passing from *a* to *h'*, or from *d'* to *k'* of Fig. 6, and as the plane at *a* is horizontal, and at *h'* is vertical, we connect, by means of a rod, one edge *b* of the plane *a* with the end *c* of a lever projected perpendicularly from the lower plane *h'*, and have an eye in the middle of the rod working on a pin *e*, which forms one of the series of pins shown in Fig. 7. Each rod may have, about midway between the middle pin *e* and each of the outer ends, a few spiral turns as shown in Figs. 6 and 8, to give elasticity in the event of the light wheel rim changing form when running upon the ground.

Fig. 7.



Now, the plane *h'* on rising to *a*, has made only ¼-revolution on its own axis;

and it makes one whole revolution only for every two made by the wheel; and, as the rod is centred on the pin *e* on a circle concentric with the wheel, and as the pin *e* for every revolution of the plane round its own axis takes twice the time to describe the circle e, e', e'', e''', e, that the wheel takes in describing the circle a, d', h', k', a, we have to restrain the motion of the pin *e* round the wheel axis to half the motion of the wheel, and do so by means of gearing connected with the motive power; but, as shown in Fig. 8, we can do so only by cutting the wheel-shaft say at the points *c* and *d*, to get this half-motion given, say at *d*, to the bar *e*, which passes through the hollow short shaft *c* to the series of crank pin-plates shown in edge view in Fig. 7.

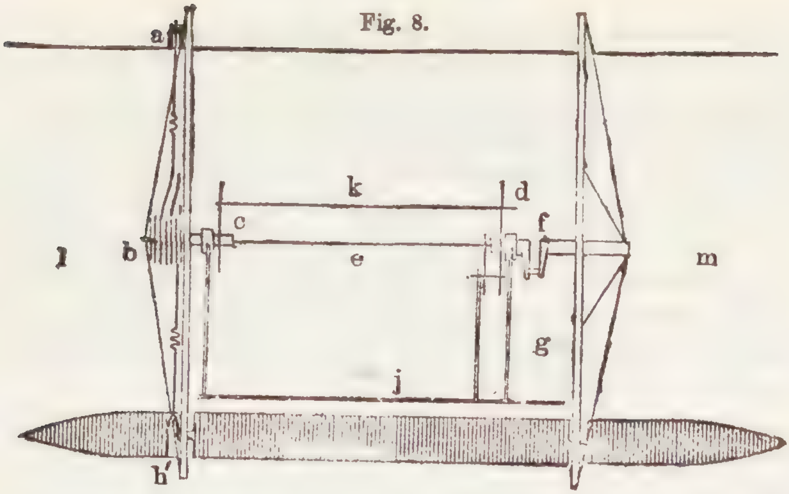
These plates are shown connected together by means of the pins alone, one pin to each interval; no further support can be got, because the rod which is centred on each pin traverses the whole face from *e* to *ee''* (Fig. 6) in each revolution of the plane round its axis, as may readily be seen were the wheel at rest, and the two planes *a* and *h'* connected by the rod *bc* rotated round their axes; and similarly, as the rod traverses the whole area between the circle described by the edges of the plane in rotating once round its axis, the ends *b* and *c* work on the pins of *V* cranks formed in the plane spindles, close to and outside of one of the two wheels, as shown in Fig. 8; as the planes are balanced on their axes, however, the axis being coincident with the centre of pressure, the connecting-rod *bc* has little stress to bear.

The cutting of the wheel-shaft is an objection, as it throws the work of keeping the frame in form upon the stiffening-rods between the wheel-rims, parallel with the planes; but if connecting-rods *b c* are employed, there is no alternative.

(50) Cords, and cord pulleys upon wheel shaft and plane spindles, in place of connecting-rods and cranks, would enable the shaft to be kept whole; but reliance could not be placed in cords to give the planes the required angles; besides the tension needed in the cords would materially increase the friction of the bearing journals.

(51) In Fig. 8, upon the suspended platform *j*, we show the position of the motive power engine at *g*. The cutting of the main shaft would throw the greater stress upon the wheel which has the engine-crank *f*, and upon the rim tie-rods which bind the two wheels together, were the motion not transmitted directly to the second wheel-shaft *c*, across the space *dc*, by means of the spur pinions and shaft *k*. We here, however, give less heed to these internal arrangements than we might were the assumed power of the planes to sustain and propel weight in air established experimentally.

Fig. 8.



(52) The machine in motion, with a man inside, would have somewhat the appearance of one of those whirling wire cylinders seen sometimes attached to mouse-cages, whatever the man inside might think; however, these cylinders did in nowise originate the idea. Access to the interior can be got by having one of the middle planes between the wheels squarely hinged at one end, and securable by a cotter at the other.

(53) Assuming it had the power of flight, a wheel constructed simply as described could go on a straight course only; and as the rotation of the wheel-frame round its axis, and the crank-bends in the line of shaft at b and f, prevent any connexion with steering appliances outside of the frame in the spaces l and m, a plane, hinged vertically in the manner of an ordinary rudder, inside the frame, and free to move to the side on which the steering pressure is required, might be found to give sufficient steering power.

(54) Were the rotation of the wheel stopped, with the planes maintained at the angles shown in Fig. 1, the wheel would at once begin to descend to earth, because the "work" stored up, or accumulated in the weight, and due to the previous Y rate of velocity, would quickly be consumed by the resistance of the air upon a displacement area so great (*par.* 47); so that there could be no quiet floating such as is witnessed in the case of large birds with outstretched wings; the wheel, therefore, must be constantly rotating if constructed simply as here described, and its Y velocity on the air path can never quite equal the velocity it would have upon a solid path or rail (*par.* 36).

(55) With the planes connected by means of the rods shown in Fig. 6, and rotating on their own axis, it is not a simple matter, for quiet floating, to devise handy means to make them slope uniformly in the direction Y; nor are the conditions much simpler for the means to keep the same edge of the plane always the leading or front edge; in which case the plane would not rotate upon its own axis, but, starting horizontally at a, would oscillate a certain number of degrees in assuming a variable slope for Y pressure, on the falling curve aa'', and be horizontal again at a''; and the range of the arc of oscillation would be ruled by the pressure required to overcome the resistance to the Y motion employed.

In connection with Fig. 9 we shall presently speak of planes made to oscillate thus, and will endeavour to show their force when thus in action.

(56) To keep the planes of Fig. 1 off the ground, when the diameter of the circle in which they rotate is 8ft., the outer rim of the wheel may be 10ft., which will keep the planes 6in. clear of the ground. In running along the ground this 10ft. circle will in one turn run a length ha'' (Fig. 2), measuring 31·416ft., whereas the 8ft. circle in one turn would run only 25·13ft.; but as the spaces between the successive points a, b, c, d, &c., in the smaller circle, occupy the same time in the motion round the axis of the wheel as the corresponding points in the greater circle, we have the plane at the highest point a still horizontal, and at the lowest point h' vertical; and, as the common centre of the wheel is possessed of Y motion at the 31·416ft. rate, and the planes have rotation motion at the 25·13ft. rate, we have the curve flatter than when the wheel is in flight off the ground on a free air path, and have the velocity that gives the edge resistance (*par.* 38) increased by the extent of the difference 31·416 — 25·13 = 6·28ft.; and in the middle of both the rising and the falling curves ha and aa'', have the planes dragging at the expense of the motive power; but as the drag is on the lower face in the rise from h to a, we have here an upward drag-pressure counterbalancing the downward drag-pressure on the upper face in the fall from a to a''; and, further, as we have in the extended air bed of the longer flattened curve the inertia of a greater bed of air, we have the balanced drag in part compensated by the greater X support at a.

CHAPTER VII.

(57) We will now multiply the pressure for 1ft. length of plane by the number of feet length assumed in *par.* 39; add the force required to overcome the journal friction; and the friction of the engine—assuming steam to be the motive power; thereby to get the total weight of constant force required in the engine. Then, for a given pressure of steam, we will find the area of piston that will supply motive power to overcome this resistance; and the number of strokes of this piston that will expend the steam formed from 1 cubic foot of water at the given pressure, will give the number of turns of the wheel per 1 cubic foot of water evaporated, and the power of the engine if derived from steam actually.

(58) As the edge area of the planes is a resisting surface altogether unproductive of useful effect, we have it acting simply as a drag upon the motive power, and therefore a consumer of the Y pressure exerted by the face of the plane; and as only 0·8435 of the whole edge resistance equal 1·0, is directly opposed to Y motion, we have, for the assumed 0·0417 square foot edge area per foot of plane (*par.* 39), 0·08419lb. × 0·8435 = 0·071lb. to be overcome Y-ward by an equivalent part of the Y propelling power; then, as the inertia force of 0·071lb. at 35·46ft. velocity is 0·078lb., and we have 180ft. of edge, 0·078 × 180 = 14lbs. acting against Y air inertia; further, as the inertia force of 0·2695lb. at 15·98ft. velocity is 0·1337, and we have say 180 square feet, 0·1337 × 180 = 24·06lbs. resistance approximately representing the stability of the air as an abutment to the Y pressure, the drag due to edge resistance tending to produce a curve slightly of the nature of Fig. 3, so that 24·06 — 14 = 10lbs. Y impulse free to overcome the less rate of resistance of the wheel-rims and arms, and of the bulk of the dead weight on the platform. The motive power the while has 0·08419lb. × 180ft. of plane edge = 15·15lbs. for the whole curve haa'', to be added to 0·4234lb. × 90ft. of plane face in aa'' = 38·1lbs.; so that 38·1 + 15·15 = 53·25lbs. constant air resistance on the planes. In Chapter XIII we shall endeavour to show that in relation to X the propelling impulse is in the falling curve, meanwhile we may assign values to certain of the forces.

In the natural gravitation of the weight earthward, the whole bed of air in the curve will act in supporting the weight, and the action of the planes will be that of sliding over an elastic bearing surface; and as the 15·98ft. velocity is about one-half the natural standard rate for one second, with the force of inertia developed to about one-half the

weight, we found it convenient to represent the sustaining power by the whole pressure of one side, viz., the planes in the falling curve *aa.*"

The mean time per shift of plane when the wheel makes one revolution per second is 0·0336 second; the velocity due to a natural fall in this time is 1·08ft.; we assume the weight of body to be 281lbs., which is 1·56lbs. inertia per square foot, and as the velocity in air to give pressure equal to this so as to produce uniform motion is very nearly the velocity of free gravitation in space in one second, say $32\frac{1}{8}$; moreover, as at $32\frac{1}{8}$ft. velocity for one second, the force of inertia rises to equality with the weight of body, and would be greater than the weight of body if the velocity rose higher than $32\frac{1}{8}$ft., and would therefore in a start from zero require force additional to the force of gravitation, we have as follows: $\frac{1\cdot 00 \text{ sec.}}{29\cdot 78 \text{ shifts}} = 0\cdot 0336$ second per shift: in this time the natural fall from zero of any weight is about 0·018ft., and the velocity due to this fall about 1·08ft., and we employ these quantities as standards.

Then, as we assume (in the absence of experimental data to determine more particularly) that in sliding over the extended bed of air the planes in one second of time fall a space which is a multiple of the mean unit fall in the unit fraction of time for one shift, we have—$0\cdot 018 \times 29\cdot 78$ shifts $= 0\cdot 536$ft. fall in one second, the natural standard velocity due to which is about 5·87ft., which we assume to be the uniform rate of the earthward tendency when resisted by the shifting of the planes.

sq. ft.

$0\cdot 843\ (0\cdot 00737 + 0\cdot 0016 \times 5\cdot 87^2) = 0\cdot 0526$lb. constant pressure of resistance per square foot of plane; $0\cdot 0526 \times 180$ sq. ft. $= 9\cdot 48$lbs. constant force assumed to be in uniform action from zero for the given area of planes, which are here for the moment assumed to be falling vertically, and supported by a bed of air equal to their own area only; then $9\cdot 48 \times 29\cdot 78$ shifts $= 282\cdot 3$lbs. aggregate for one second of time, and this is equal to the inertia force resisting the planes in the rotation of the wheel.

(59) Next, as the sustaining pressure X per square foot of area is 6·30lbs., due to the inertia of the air travelled on (*par.* 22), we have 6·30lbs. × 90ft. in *aa*″ = 567lbs., because we have the resistance of as many columns of air as there are shifts of plane, which makes the work performed in one revolution of the wheel a multiple of the force of the unit shift of plane at the given X velocity; this, however, represents the weight of pressure, whereas the inertia force is less, thus

$\frac{567}{32 \cdot 18} \times 15 \cdot 98 = 281$lbs. sustaining power of the air resistance, and assumed to be the weight of the wheel and its load. This weight is sustained only by the action of the planes over an extended bed of air, with the constant X resistance equal to 38·1lbs. as felt by the motiv power; consequently, in the form of friction due to the motive o propelling power, in the journals of the plane spindles, we have only the above $38 \cdot 1 + 15 \cdot 15 = 53 \cdot 25$lbs. resistance; and this will be in part reduced by the gravity of the weight of the planes acting in opposition to the upward pressure of the air resistance in the planes. And as regards the part the planes have in the friction due to the whole pressure $= 567$lbs. sustained, we have it in the friction between the surface of the plane and the air-bed travelled on, which may here be neglected (*par.* 8). But as the weight of the wheel-frame, and the load upon the platform inside, is borne by the planes, and therefore produces journal friction on the plane spindles that support it (further, as the weight of the planes is here undetermined, and as the power needed at the engine crank to overcome the frictional resistance is very small relatively), we will assume that the whole pressure is producing friction.

(60) And, employing the ordinary value of journal friction, viz., 0·07 of the weight, we have $567\text{lbs.} \times 0 \cdot 07 = 39 \cdot 69$lbs. resistance in the journals; and allowing the crank leverage *ab* (Fig. 6) to be equal to the leverage of the engine crank = 6in., and the diameter of the plane spindle to be $1\frac{1}{2}$in., we have $6 \div 1 \cdot 5 = 4$ times; and $\frac{39 \cdot 69}{4} = 9 \cdot 92$lbs. power needed at the pin *e* of Fig. 6, coincident with the engine crank-pin, to overcome the resistance in the journals.

(61) Now, as the radius leverage of the engine power is only 6in. for a 12-inch stroke, whereas the plane pressure on the wheel-rim has a radius leverage of 4ft., the engine power will require to be as much greater than the plane pressure as its crank leverage is less; so that $4\text{ft.} \div 0 \cdot 5\text{ft.} = 8$ times; and $53 \cdot 25\text{lbs.} \times 8 = 426$lbs., to which we add the 9·92lbs. of journal resistance, making the total resistance at the engine crank 435·92lbs.; and, making the ordinary allowance of $\frac{1}{7}$th of the piston pressure for power consumed in the engine, we have for total actual piston pressure needed, $435 \cdot 92 + 62 \cdot 3 =$ say 500·0lbs. piston pressure.

(62) Then, taking steam at 20lbs. effective pressure above the atmospheric pressure per square inch of piston area, we have

$$\frac{500\text{lbs. resistance}}{20\text{lbs. steam}} = 25 \text{ square inches area of piston.}$$

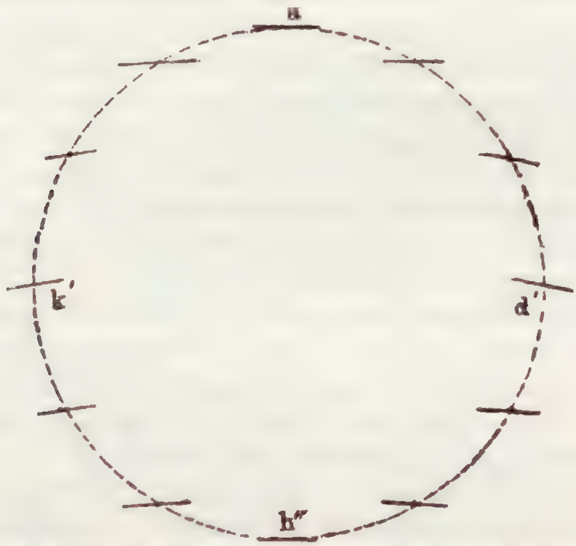
(63) The stroke of the piston being 12in., we have in each stroke a consumption of 0·173 cubic foot of steam, which is equal to 0·346 cubic foot for each full turn of the crank; and as the relative volume of steam at 20 + 15 = 35lbs. pressure is 765 to 1 for the water it was formed from, we have from every cubic foot of water evaporated steam for 2211 turns of the wheel; which, supposing the wheel were running on a path that allowed no slip, would, at one turn per second, give a distance of 10·52 miles in 36·85 minutes = 0·614 hour; and, as 1 cubic foot of water evaporated per hour is the value of 1 horse-power nominal, we require here $\frac{60 \text{ minutes}}{36·85 \text{ minutes}}$ = 1·63 horse-power in engine

To evaporate 1 cubic foot of water, the ordinary estimate allows 9lbs. of coke; but as a cubic foot of water weighs 62lbs., and as the boiler that evaporates 1 cubic foot of water in 36·85 minutes cannot be a small one, it is clear that the motive power cannot be steam produced in the ordinary manner; and we have employed steam pressure here merely as a standard index of the power required.

Chapter VIII.

(64) We shall now endeavour to determine the value of the pressure of the planes in the form shown in Fig. 9, in which the plane

Fig. 9.



does not rotate round its own axis, but simply oscillates on an arc of say 25°, to give Y pressure in its passage from a to a''; the area of

X pressure being horizontal at both the points a and a'', unless when otherwise purposely sloped to get a rising or a falling resultant, by the slight canting of the platform inside, or of the framing that supports the eccentric ring.

(65) The axes of the planes when rotating with Y motion will describe a curve, which will have its point of rest at a'' (Fig. 4) as in the curve (Fig. 2) for the form of wheel (Fig. 1); but, supposing the planes able to exert the required Y power, and to travel in the path of their inclination, so as to form the curve thereby, we might expect to find the curve flatter than shown in Fig. 2, and the distance $h''a''$ consequently greater.

(66) Were the planes kept uniformly horizontal, and the wheel to run upon a rail, the curve would be the same as for the form (Fig. 1), so long as the rail was run upon; but the planes thus horizontal could exert no Y pressure, and it is clear that the rising and the falling curves would be in opposition.

(67) While upon the rail, the mean angle $h''aa''$ (Fig. 4) for the curve, would be about $57\frac{1}{2}°$ (*par.* 32) the cosine value of which is about 0·8435; but, as the planes are here for the moment assumed to be uniformly horizontal, so as to be of the full value 1·0 in resistance to X, we have the 0·8435 value, relative, not to area of X displacement as in form Fig. 1 for the curved resultant ada'' (Fig. 2), but to the rate of velocity in shifting on to fresh air, represented by the decreasing successive spaces vb, $b''c$, $c''d$, $d''e$, &c. (Fig. 4), in successive equal spaces of time, which are best represented by the spaces which separate the axes of the planes on the wheel's circumference. But we have the value of the resistance to X in this 0·8435 measure only while the wheel is running along the rail; and if, on an air path, we can increase the Y velocity beyond the velocity of rotation, and thereby make the inclined planes shift on to new air more rapidly, we may raise the mean X resistance nearer to the full value 1·0.

(68) The arc of oscillation measures say 25°; the mean of this for the curve aa'' is $12\frac{1}{2}°$, and $57\frac{1}{2}° + 12\frac{1}{2}° = 70°$ for the angle which, with a longer Y base, is to take the place of the angle $h''aa''$, with the shorter Y base $h''a''$. For convenience, in treating this arbitrary angle, we will employ the letters belonging to the angles of Fig. 4.

	Sine $h''a''$.	Cosine $h''a$.	Cotangent gw.
$h''aa'' = 70°$	0·93969	0·34202	0·3639

$$\frac{0{\cdot}34202}{0{\cdot}93969} = 0{\cdot}3639.$$

And, as at this angle, the sine $h''a''$ in the time of the plane's descent from a to a'', represents the resistance of the plane to X, we have $h''a$ representing the pressure which would give Y motion were the plane actually moving in the line of the tabular resultant apa'' with a uniform slope of 70° vertical angle, coincident with the angle of the resultant.

(69) But the mean angle giving Y pressure is only $12\frac{1}{2}°$ horizontal angle, say $aa''h''$, equal $77\frac{1}{2}°$ vertical angle, say $h''aa''$; and the sine and cosine of this latter angle are—

Sine......... $h''a'' = 0{\cdot}97629$
Cosine ...... $h''a = 0{\cdot}21643$
Cotangent.. $gw = 0{\cdot}22169$
Tangent ... $yx = 4{\cdot}51$

So that the Y pressure cannot be in greater proportion than 0·21643 to 0·97629 for the X pressure; and as the sine $h''a''$ of the mean angle is 0·97629 to 1·0 for the actual Y length of the plane, we make the Y length of the plane when horizontal equal to the cosecant of the $77\frac{1}{2}°$ vertical angle, that is, 1·02427ft. in place of 1ft.; and thereby have the sine $h''a'' = 0{\cdot}97629$ in the Table, representing 1ft. actual; so that, as the tangent gw bears the same proportion to the radius $a''a$ that $h''a$ bears to $h''a''$, if $h''a''$ the sine $= 0{\cdot}97629$ be taken to represent the tabular radius 1·0 for X pressure, $h''a$ the cosine will represent the cotangent 0·22169 part of 1·0 for Y pressure; consequently, if the X pressure be 1lb., the Y pressure will be 0·22169lb.

(70) This refers to displacement resistance. The velocity of rotation on the rail gives the curve ada'' for the mean angle of about $57\frac{1}{2}°$, with the $h''a''$ sine value of 0·843, and the $h''a$ cosine value of 0·537; and

$$\frac{\text{sine } 0{\cdot}843}{\text{cosine } 0{\cdot}537} = 1{\cdot}57 \text{ ratio} = \text{tangent } yx,$$

so that, as $h''a$ is of the constant value of 8ft. for the wheel's diameter, measured between the opposite axes of the planes, we have $1{\cdot}57 \times 8 = 12{\cdot}56$ft. for $h''a''$, the distance run upon the rail

As, however, the $12\frac{1}{2}°$ mean Y angle increases the mean angle of the curve to 70°, we have for 70°

$$\frac{\text{sine } 0{\cdot}9396}{\text{cosine } 0{\cdot}342} = 2{\cdot}747 \text{ ratio} = \text{tangent } yx,$$

and $2{\cdot}747 \times 8 = 21{\cdot}976$, say 22ft., for $h''a''$, the length that would be travelled in a half-turn of the wheel upon the air path free of the rail, were the Y pressure in sufficient force, and to take full effect in producing Y motion.

(71) We shall now ascertain the value of the air resistance to Y,

and assume that the velocity of rotation round the axis of the wheel is at the rate of one full turn per second; the valne of the mean of V^2 for X will therefore, as before (*par.* 15), give 17·58ft. for V; and the air resistance to X at this mean rate will be 0·502lb. per square foot of projected area, perpendicular to the direction of the motion (*par.* 18); and, as we make the actual length of the plane 1·02427ft., we have the mean sine $h''a''$ equal to 1·0 for the area of displacement in the descent from a to a''; then, per foot of plane,

0·502lb. × 0·22169 = 0·11128lb. proportionate Y pressure.

(72) We have assumed 180 square feet as the whole area of the planes, and, as Y pressure is had only on the falling curve aa'' when the drag due to edge resistance is neglected, that is, on one-half of the area for the whole curve, we have

0·11128lb. × 90ft. = 10·015lbs. constant mean Y pressure.

(73) The edge resistance of the planes would be due here to a velocity, roughly, of about 44 — 25·13 = 18·87ft. difference + 35·46ft. of *par.* 38, = 54·33ft., which would give a pressure of 4·665lbs. per square foot of area perpendicular to the direction of the motion; and, as for the 0·0417 square foot of edge per foot of plane (*par.* 39), we have 0·0417 × 180 = 7·5 square feet, and 7·5 × 4·665 = 34·98lbs. resistance, for which there is only 10·015lbs. Y power.

(74) It is clear, therefore, that the Y pressure of the planes, when oscillating on a mean arc of 12½°, is insufficient to propel the wheel; and if the angles of inclination given to the planes in the rear or rising side of the wheel correspond, because of the mechanical difficulty to have it otherwise, to the angles in the falling or propelling side, the upper face resistance on the rear planes will neutralize the X effort of the front planes more and more as the Y distance $hh''a''$ for one turn is shortened.

(75) Greater velocity of rotation would develop more Y pressure, but the plane edge resistance would develop with it, and in this edge resistance of the rotating planes do we see one great disadvantage to the wheel form.

(76) It is evident, moreover, that the eccentric or other gearing that can vary the angle of the planes in the rising curve so as to travel in the line of a curved path of varying Y velocity, though it must necessarily be of a simple nature and of light construction, presents considerable difficulties, more especially when, to have the wheel under perfect control, the simple mechanism must be capable of bringing all the planes to slope uniformly in any direction; horizontal for quiet floating; vertical for arresting motion; or inclined up or down to the horizon.

CHAPTER IX.

(77) In Fig. 10 is shown an arrangement of eccentric rods, by means

Fig. 10.

of which the face of the wing planes may be horizontal at both top and bottom of the wheel, the *Y* propelling force being got by means of the variable angles of inclination given as shown in the descent from *a* to *h*.

The floats are formed, say, of thin veneer, to which silk or other cloth fabric may be glued.

The *V* cranks, placed in the planes as shown in Fig. 8, are connected together by means of a series of light rods, each of which has a few spiral turns in the middle of its length for elasticity.

To one of the plane *V* cranks thus connected, say to the crank *Z*, one end of a stiff rod is hinged; the other end *K* is weighted, and at an intermediate point in the length, the rod works on the pin *B* of the oscillating crank *A B*.

The crank *A B* is made to oscillate in a determinate manner by connection with the motive power that causes the wheel to rotate; and

the oscillation is needed to allow the end Z to occupy the successive positions f, h'', i, j, k, l, *&c.*

Only one oscillating rod can be got to work, and this single rod is all there is to keep the system of light spiral rods in form.

The mean angle of the planes in the descent from a to h in Fig. 10 is greater than we have named for Fig. 9.

In Fig. 11 we show the planes inclined as in Fig. 10, on a curve

Fig. 11.

equal in length to the circumference of the wheel only, as in Fig. 2; so that, at the successive points, Fig. 11 exhibits the variable divergence of the angles of inclination of the planes from the line of the curved path, while the wheel is running upon the ground.

Assuming the planes have power to float the wheel, and to give Y velocity greater than when running on the ground, the angles of inclination of the planes will more closely approximate to the line of the curve, the flatter the curve becomes by the extension of the Y distance LM.

The plane N (Fig. 11) in rising will tend to depress the wheel, but the upward motion from L to N is slow, and the plane quickly assumes an angle closely approximating to the line of the curve on rising beyond N.

The wing-planes between the wheels (Fig. 8) may be of the hollow form shown in Fig. 10, but the extension outside of the wheels may be of a form possessing greater elasticity.

(78) It is apparent to sight that the sum of the circular lengths of the eight rods which connect together the V cranks of the planes is only a very little less than the circumference of the circle which passes through the axes of the planes; and that if the axes of the planes be free to move a very short distance in toward the axis A, the length of the circle thus contracted may be brought to equality with the circular value of the sum of the lengths of the connecting-rods, the polygon formed by

which will then be free to change from its general oval form to a form nearer a circle, if not prevented by the oscillating crank-rod KZ; and in this change of form the V cranks of the planes will be allowed, approximately, to take the same vertical direction all the more readily if the planes be set upon the axes so as to give a slight excess of pressure on the rearward part of their surface; and with the planes thus at liberty to assume a general horizontal direction, the wheel may be quietly floated at the will of the person on the platform.

The means employed to move the axes of the planes the very short distance inward are not shown in the Figures; it is evident, however, that those means would require connection with the oscillating crank of the main rod KZ, and be capable of being thrown into and out of gear in the short time required to move a simple hand lever.

Chapter X.

(79) As the edge resistance of the planes rotating round a centre is greatly increased by the velocity of rotation, we shall now speak of planes which shall only oscillate on hinges in the manner of birds' wings.

Let the weight borne be 2·5lbs. per square foot of wing.

According to Morin, the velocity required to give 2·5lbs. resistance in air is—

$$\text{Area } (\cdot 00737 + \cdot 0016\,V^2) = R, \text{ and } \overset{\text{sq. ft.}}{1}\ (\cdot 00737 + \cdot 0016 \times 39\cdot 52^2) = 2\cdot 5\text{lbs.}$$

Then if we represent this 2·5lbs. resistance by a column of air at rest weighing 2·5lbs., this column of the given weight, if put in motion from a state of rest by pressure increasing beyond the pressure that it balances, would offer the resistance of its inertia increasingly from zero; and, as we are treating of the resistance of air, with a plane of 1 sq. foot area, and weighing 2·5lbs., it is clear that as the air resistance due to a velocity of 39·52ft. per second would balance the plane possessed of that weight, so as to make the motion uniform, we would have the weight sustained at a given level in the air, by giving the velocity to wing-planes oscillating upon hinges on the weight.

The velocity of 39·52ft. per second is in the ratio of $\frac{39\cdot 52}{32\cdot 18} =$ 1·228 to 1 for the velocity due to natural gravity for 1 second, or would take 1·228 seconds to develop naturally, so that as the elements of natural gravity form the standard by which to determine the value of weight in motion generally, and as we take the rate per second as the

unit standard, and further, as we have to consider the winged body as tending at any point of its assumed horizontal flight to fall earthward from a horizontal level representing, as regards earthward motion, a line of rest, we employ the force of gravity of the given weight for one second of time t (the velocity generated in one second in space free from air is $g = 32\frac{1}{6}$ft., irrespective of the value of the weight), and have in $\frac{W}{g} \times \frac{v}{t} = \frac{W}{32 \cdot 18} \times \frac{32 \cdot 18}{1}$, the force of gravity equal to the weight simply, and equivalent to the force of inertia resisting it, which thereby defines the value which we call weight. And when the motion $\frac{v}{t}$ is less than for natural gravity, it is obvious that the weight being urged with less speed-force, its inertia force is less developed, and, when the motion takes place in air, with the given weight centred say in the axis of the hinge on which the wing is made to work, and the earthward motion that would otherwise be in the weight is substituted by the motion given to the wing, so that the weight is sustained and the wing alone moves, it is evident that the air must now be made to sustain the weight, by means of the pressure equivalent to weight developed in it by the wing-strokes, and as the 39·52ft. velocity gives 2·5lbs. resistance, represented by an R column of that weight at rest, any less velocity will give a column of R resistance proportionately lighter, so that as the force of inertia of the air is the sustaining force, the force to sustain the given 2·5lbs. weight must be got in the one second of time, from as many of the lighter R columns as will form an equivalent to the standard single column, and this can be got only by shifting the plane forward horizontally while the wing is beating vertically.

(80) As g is the standard value of the force of gravity for one second, and as the time in the expression $\frac{v}{t}$ is one second, we are here free to simplify the equation $\frac{W}{g} \times \frac{v}{t}$, thus $\frac{W}{g} \times v = Mv$ for 1 second. Were the times t in the several cases unequal, we would require to employ the full equation, as the forces developed are proportional to the rates of motion for a given time.

Were the weight W in space free of air, its natural force of gravity would take, as before observed, $\frac{v}{g} = \frac{39 \cdot 52}{32 \cdot 18} = 1 \cdot 228$ seconds to attain the velocity that in air would develop 2·5lbs. resistance, and as we assume W to weigh 2·5lbs., this air resistance would simply balance it; but as v when thus employed in developing air resistance would be

the actual rate per second, without reference to the time that natural gravitation would require to develop it, it is clear that the force here performing in one second the work that requires 1·228 seconds in natural gravitation, it exceeds the natural force of gravity, thus—

$\frac{2\cdot5\text{lbs.}}{32\cdot18} \times \frac{39\cdot52}{1} = 3\cdot07$lbs. force, which is 0·57lb. in excess of the natural force.

As before observed, were $\frac{v}{t} = \frac{32\cdot18}{1}$, the force—which in natural gravitation may be regarded as either the force of inertia in the weight, or the force of attraction drawing it earthward—is the weight simply, and for lower velocities per second we have the force developed proportionally less.

(81) Now we will, for the moment, assume that in giving the wing while beating 7 shifts horizontally on to fresh air at rest, we get 7 times the support, then,

$$\underset{\text{sq. ft.}}{39\cdot52^2} = \frac{1562}{7} = 223 = V^2, \text{ and } \sqrt{223} = 14\cdot933\text{ft.} = V.$$

Then, 1·0 (·00737 + ·0016 × 223) = 0·364lb. air resistance, which we represent as being balanced by a column of air weighing 0·364lb., which we term w, and employ it in the equation for the force of inertia to get the resistance of inertia which this weight w would oppose to v at the 14·933 rate. $\frac{0\cdot364}{32\cdot18} \times 14\cdot933 = 0\cdot169$lb. inertia force of resistance to the pressure of the wing; and as we assumed 7 horizontal shifts, which gives 7 columns of resistance, we have 0·169 × 7 = 1·183lbs., which is less than half the sustaining resistance wanted, so that $\frac{2\cdot5}{1\cdot183} = 2\cdot11$ times, and 7 shifts × 2·11 = 14·77 shifts needed at the given 14·933ft. rate of velocity, to sustain the 2·5lbs. weight in horizontal flight.

Assuming the wing to be 1ft. broad, 14·77 shifts per second would require the weight to be propelled horizontally at the rate of 10 miles an hour upon the ground before it could be floated off, supposing the wing mean-velocity to be no greater than 14·933ft. per second.

Assuming the wing to be 10ft. long, the centre of pressure to be say 6ft. from the hinge, and the length of wing stroke measured at the centre of pressure to be 5ft., this would give $\frac{14\cdot933}{5} = 2\cdot987$ full strokes per second, or about 180 strokes per minute.

(82) Doubling the number of wings, and making each only 6in. broad, the length and velocity of stroke being as before, 14·77 shifts

would be got in half the time, and as we here have the air resistance per unit of surface the same as before, we have the inertia resistance of the double number of columns of air support for one 6in. breadth in one second of time, the same as the resistance for the 1ft. breadth shifting the same horizontal distance in one second: thus, 0·364lb. air resistance for 1 square foot, with breadth 1ft., at 14·933ft. velocity per second; then $\frac{0{\cdot}364}{2} = 0{\cdot}182$lb. for $\frac{1}{2}$ square foot, with breadth 0.5ft., at the same velocity; $\frac{0{\cdot}182}{32{\cdot}18} \times 14{\cdot}933 = 0{\cdot}08437$lb. inertia force of weight representing the pressure.

Twice the number of shifts in one second gives $14{\cdot}77 \times 2 = 29{\cdot}54$ shifts, and $0{\cdot}08437 \times 29{\cdot}54 = 2{\cdot}5$lbs. aggregate inertia force for one second, so that as there are two wings now in place of one, we have $2{\cdot}5 \times 2 = 5$lbs. inertia force per square foot of area.

(83) If we keep the number of wings, the velocity of stroke, and the area of each wing, the same as at first, but extend the length so that the breadth shall be only 0·5ft., we shall have, as in the last case, 29·54 shifts in one second, and shall thereby similarly have 5·0lbs. inertia resistance for support when the horizontal propulsion is at the rate of 10 miles an hour, or shall have inertia resistance equal to the weight when the rate is only 5 miles an hour.

A wing extended thus, however, and making beats equivalent to a mean velocity of 14·933ft. per second, would be unwieldy; but, as the 10-mile rate gives 5lbs. support per square foot area, the velocity of the wing-stroke would be proportionately less than 14·933ft. per second, to produce resistance to float 2·5lbs. off the ground. Moreover, when afloat, and free from the resistance to rolling on the ground, a greater horizontal velocity than 10 miles an hour would be attainable, giving more rapid shifts of the wing breadth on to new air at rest, so that the velocity of the wing-strokes may be proportionately still further reduced; and this implies a reduction of the motive power.

(84) In Fig. 13, with a view merely to illustrate the principles concerned, we show three wings of short length taking the place of one wing of long unwieldy length, the breadth being the same, and the united area of the three being equal to the area of the one.

While on the ground the horizontal motion that gives the wing the extended bed of air for support may be got by means of lightly-framed wheels; but, when sufficient air support is got to float the weight, the angle of the wings in the sloping of the breadth in the

manner already described will give the forward impulse; and, in the upstroke the wing with the angle reversed will rise free from pressure on the upper face.

Figs. 12 and 13 give general expression to the manner in which the wings may be worked. It is a simple matter to compute the motive power that would be required at the crank axle to work the wings against the given air resistance.

Fig. 12.

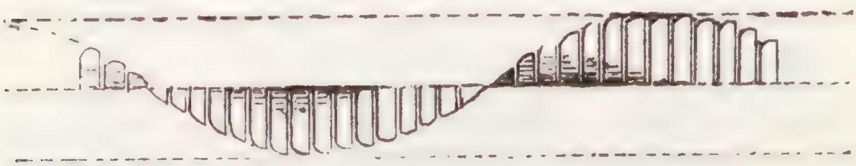
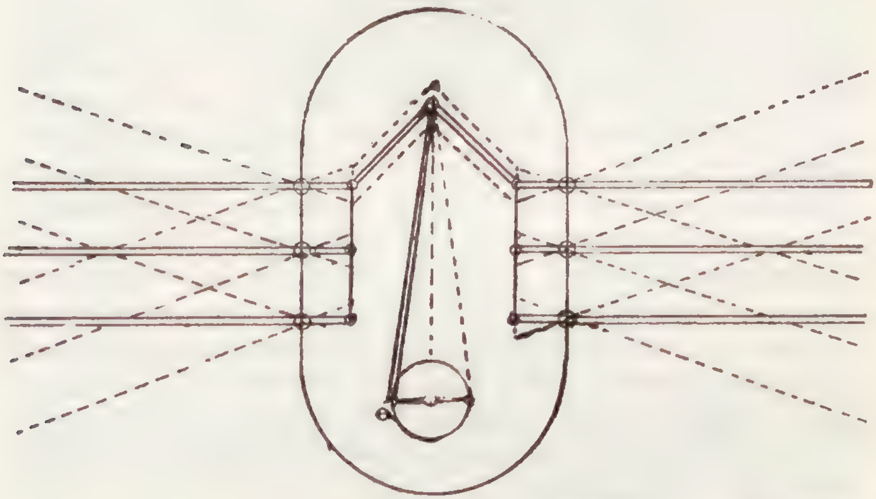
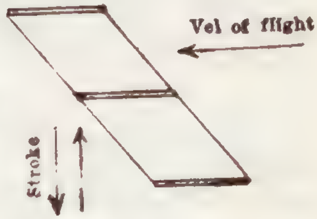


Fig. 13.





The three wings coupled thus (looked at endwise), to get resistance on all three when the velocity of flight is low at starting.

(85) Now, were the wings few in number, so that in shifting horizontally they might always be coming upon air not previously disturbed, we might get the full effect of the inertia of the bed of air which is travelled on; whereas when the wings are many, the rearward wings would find the air they came upon in a disturbed state, more especially

at the lower horizontal speeds when gathering velocity to rise from off the ground. Were they ranged in horizontal series, so as to make their up and down strokes in unison, the leading wings alone would make each successive down-stroke in new air. In the up-stroke the wings might be inclined so as to rise with front edge resistance only; but, until velocity of flight was reached sufficient to give a bed of air support so great that the inertia of the weight of this bed would sustain the winged body so as to float it, part of the force of the down-stroke would be expended in giving motion to the air, and the rear wings would be beating upon air which was in confused motion.

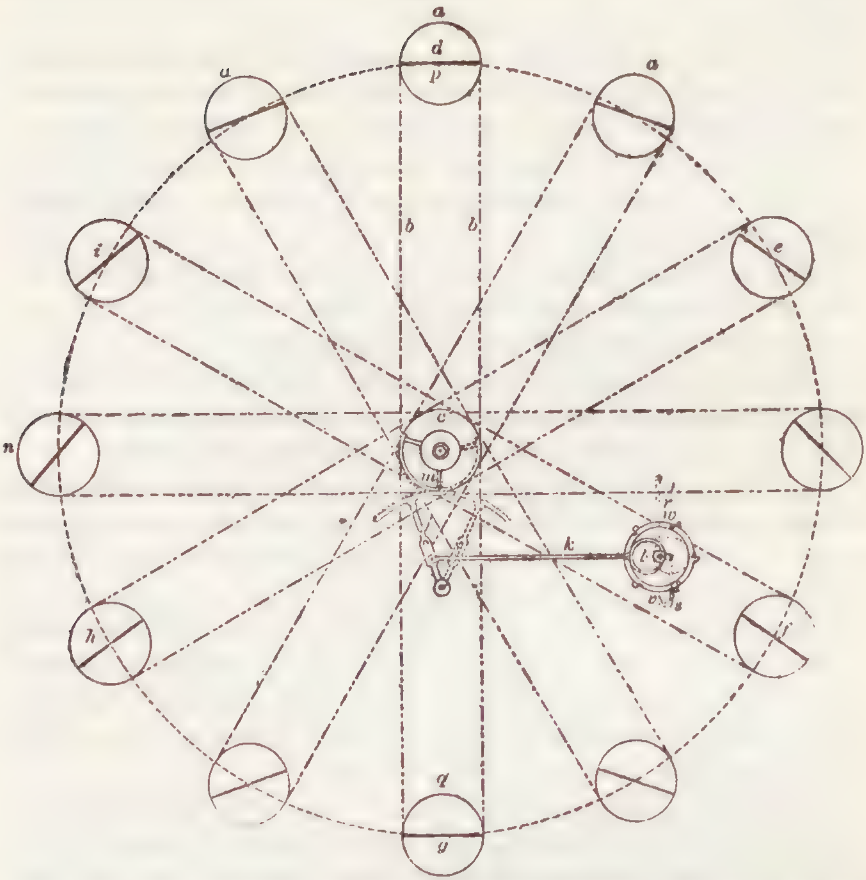
Were the wings ranged with their hinge axes on each side in one horizontal line, but the crank *e* of each single set coupled as in Fig. 13, so keyed that the tips of each series might form the curve shown in Fig. 12, we would then have the rearward wings following in the path of the preceding wings, with a result similar in kind though not quite in degree to that which would attend the employment of a single plane, equal in area to the sum of the wing area it represented, were this single plane made to oscillate in the wave-like manner represented by the curve. But as each wing of the series has to make a given number of strokes per second to get the air resistance *R* already spoken of, we would have as many wave-like curves in the distance of flight per second as there are strokes in a second; so that the curve of Fig. 12 relates merely to the placing of the cranks on the motive power driving shaft to produce continuous in place of maximum and minimum effect; and it is clear that the velocity of flight must be great to give an air-bed so extended in the direction of the flight that the leading wings, with their given weight of pressure, have not force to give appreciable motion earthward to the air that has yet to receive the weight of pressure of the succeeding or rearward wings.

Chapter XI.

(86) Fig. 14 shows the end view of the winged wheel before described, but with another form of gearing for giving a variable slope to the rotating planes, with means to bring them at will to one uniform slope for quiet floating. In this case the axle of the wheel is not cut, nor are the axles of the planes *V* cranked as in the preceding instances.

All the gearing is inside between the wheels that carry the planes. To balance the weight of the gearing one-half of the planes are connected at one ring, and the remaining half at the other ring.

Fig. 14.



On each plane axle there is placed one grooved pulley *a*, of say about 9in. diameter; an endless chain *bb* (to be hereafter spoken of) works round this pulley and one lettered *c* on the wheel axle, so that, for the six planes *d, e, f, g, h, i*, geared at the near ring in the figure, there are six pulleys *c* close set together on the wheel axle, each free to move independent of the others, and each oscillated by an independent cogged lever *j*, which is put in motion by a connecting-rod *k*, in gear with an eccentric *l*, which is worked by connection with the motive power engine.

The clump of pulleys on the wheel axle are of uniform diameter, the same as that of the single pulleys on the plane axle. Were the pulleys *c* kept fixed, the planes of the pulleys *a* would be maintained at one constant slope, say horizontal. They are wanted to be horizontal only at the top and bottom of the wheel, and the oscillation of the

pulleys *c* is for the purpose of giving the plane that variable inclination that shall produce the motion of flight.

(87) There is an eccentric *l* for each pulley *c*, and cogged lever *j*; and these eccentrics are so set upon the shaft—in the manner to be presently described—that one-half the number is oscillating the planes on the rising side, while the other half is oscillating those on the descending side. For clearness we show only one set of eccentric *l*, cogged lever *j*, and cogged quadrant *m*.

The full lined forms *j*, *m*, and *l*, show the position for the plane at *n*; the dotted forms show the position for the plane at *o*; and the mean of those two positions gives the plane at *p* or *q*.

The pulleys *c* are close to the inner face of the wheel ring, and are fixed on hollow tubes, concentric with the wheel axle, and of different diameters, so that the larger easily slide round upon the smaller.

At one end of each tube a pulley *c* is fixed, the inner tubes severally project at the other end beyond the outer tubes, sufficiently to receive on each projected end a light cogged quadrant *m*, and with this quadrant is the cogged lever *j* in gear.

The eccentrics *l* have a narrow seat upon the shaft, but their work is light, and instead of being keyed to the shaft they are driven by a small shallow cog which projects from the shaft, and which has a play of one-half the circumference, from *a* to *b* in the recess cut in one end of the eccentric boss, as shown in Figs. 16 and 17.

According to the direction of the motion, this cog will catch at one end or other of this recess, and when the motion is stopped, and it is desired to bring all the crank levers *j* and quadrants *m* to the mean position, so as to get all the planes say horizontal, the eccentrics are slid round in the same direction they had before been driven in, the cog is thereby left behind somewhere in the recess, at a distance from the end it had been pressing against determined by the position of the eccentric when the axle motion ceased.

To work the eccentric thus free, a light thin ring may be attached to the outer point of the eccentric, so as to be concentric with the axle; on this ring are two small projections on opposite sides of the diameter as shown. Then, a crank lever *r*, working freely on the axle, has attached to the crank limb a hanger *s*, which on the motion of the hand-bar *r* is raised to rub on the edges of all the thin rings, so that, on the continued motion of the hand-bar round to *u*, all the projections between *v* and *w* of the several rings are carried up to *w*, and this will cause all the planes to be sloping uniformly.

Fig. 15.

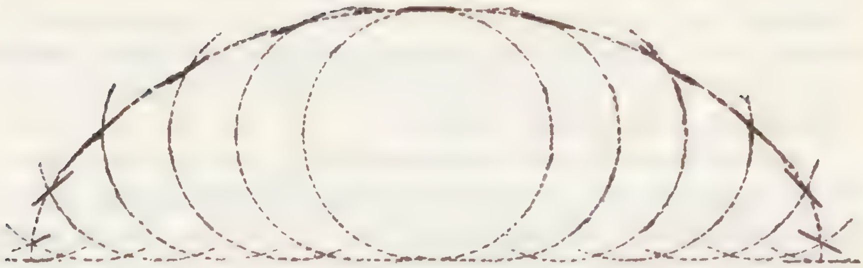


Fig. 16.

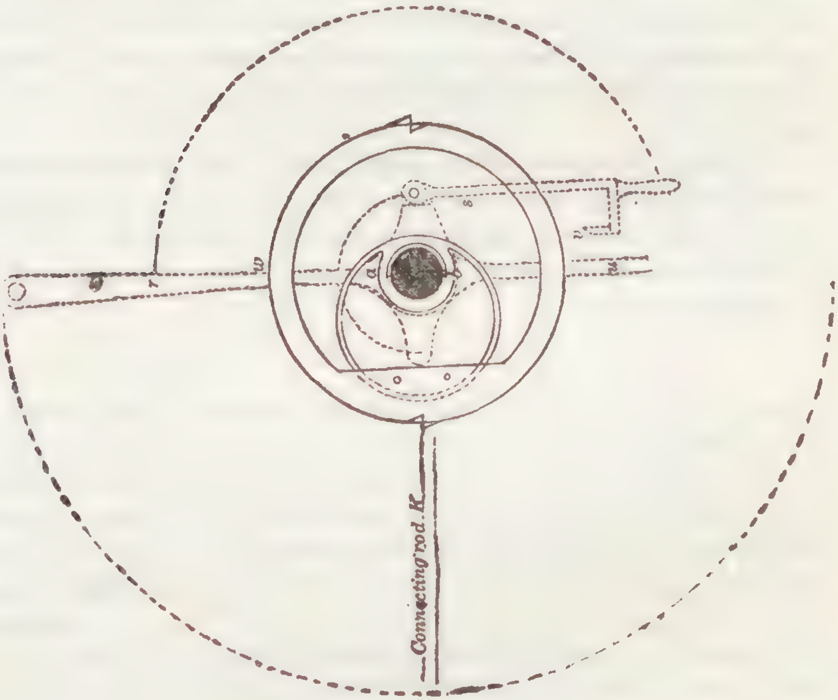
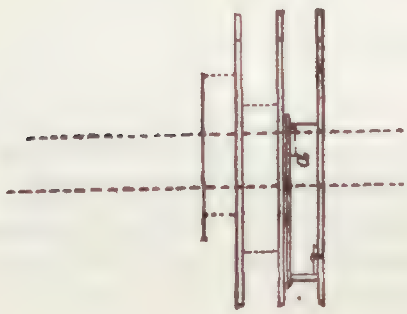


Fig. 17.



The endless chains *bb* must have a reliable hold upon the pulleys; this might be got by means of long links and of flats to correspond in the pulley groove. It may also be got by casting small iron balls between the eyes of links at close intervals; these balls to lie half bedded in corresponding hollows in the groove.

(88) The force required in the chains to regulate the inclination of the planes is very small, but though the gearing be made correspondingly light, the aggregate weight will be a burden upon the sustaining power of the planes; and the gearing here described is to be regarded (equally with the wheel form of the plane-frame) only as suggestive data from which something simpler may be devised; and with a view to lightness, thin sheet steel, in the room of the veneer mentioned in *par.* 77, might be employed, as spindles the full length of the planes might thereby be dispensed with.

The slot in the arm of the cogged lever is to allow of the arc of oscillation being regulated to suit the conditions of flight; a very simple addition would enable the arc to be regulated when in motion.

Fig. 15 shows the angles the planes would form with their path in a cycloidal curve.

FALLING PLANES.

CHAPTER XII.

(89) Suppose a thin plane, square, and of 1 square foot area, at rest in air, as at *a* in Fig. 18. If allowed to fall freely earthward, its velocity

Fig. 18.

of descent will be accelerated variably until the rate of 25ft. per second is reached, beyond which point, in air of ordinary density, the rate will continue constant at 25ft., because at that rate of velocity the plane meets with resistance equal to its displacing pressure, and the force of inertia developed in the resistance which the air offers to displacement is equal to the force developed in the weight of the plane in the motion of its fall.

The fall required by gravitation close to earth to generate 25ft. velocity per second is 9·71ft., equal to *ad* in Fig. 18; and we employ this height of fall as a standard by which to determine the relative values of different velocities.

The force of inertia of 1lb. falling with the velocity of 25ft. per second is $\frac{1\text{lb.}}{32{\cdot}18} \times \frac{25\text{ft.}}{1\text{ sec.}} = 0{\cdot}7767\text{lb.}$, and, though the pressure is equal to 1lb., the sustaining resistance of the air displaced under this pressure is only 0·7767lb. Were the sustaining resistance 1lb. equal to the pressure, then the motion of the plane would be arrested.

(90) We will now suppose that the plane is possessed of horizontal or *Y* force to carry it along from *a* to *b* at the uniform rate of 25ft. per

second, so that it makes 25 clear shifts on to air at rest in that distance and time.

The time per shift is $\frac{1 \text{ sec.}}{25} = 0{\cdot}04$ second; but the sustaining force of inertia required to place the falls x and X on equal terms as regards support is $\frac{0{\cdot}7767\text{lb.}}{25} = 0{\cdot}031\text{lb.}$

(91) Were the velocity which is generated in a fall from zero in 0·04 second of time to develop air resistance equal to the falling force in the weight of plane, we would have the resisted fall x for the given plane and Y distance equal to only 25 times this first-shift fall; but as the sustaining force on the Y path is not the imposed pressure, but the inertia of the air displaced, or the resistance which it offers to any acceleration of the velocity to which the pressure is due, the plane, if assumed to fall from zero or a state of rest at a, would have to fall at a variably accelerated rate a longer time than 0·04 second to give pressure that would yield the requisite 0·031lb. resistance to acceleration referred to above.

When the plane falls in a purely vertical direction X, the resistance of the opposing single column of air is able to balance the force in the falling plane so as to produce uniform motion, only when the velocity rises to 25ft. per second, and then requires this velocity to be maintained in the plane, which is consequently carried at that rate earthward; and we seek to lessen this inconvenient velocity by carrying the plane on a Y path over the surface of a greater body of air, thereby extending the area of support.

Then, in the case of a plane making a free fall in air from zero, in place of one column of one second's length, we have 25 columns of $\frac{1 \text{ sec.}}{25}$ mean length; and as the weight of air that forms these minor columns is come upon at rest, with the velocity of the falling plane increasing, but with the rate of acceleration decreasing as the resistance develops, we have the force of inertia in each succeeding column variably greater than in the last preceding; but in the sum of the resistances of the series of minor columns we have the sustaining resistance of the air equal to the sustaining resistance of the long single column for a purely vertical fall. The decrease in the rate of acceleration as the falling plane develops increasing resistance in the opposing air when shifted Y-ward cannot well be stated in simple terms, but we may get the result wanted, approximately, in ratio form, thus :—

The force of inertia in 1lb. falling at the rate of 25ft. per second is, as before observed, 0·7767lb., and $\frac{0·7767}{25} = 0·031$lb. constant force of inertia on the Y path of 25 shifts of place, and a velocity of about 8·3ft. per second will give this.

The natural fall by gravity in space, to generate this velocity is 1·07ft., and $\frac{9·71\text{ft. for } 25\text{ft. velocity}}{1·07} = 9·0$ ratio, or $\frac{25^2}{8·3^2} = \frac{625}{68·89} = 9·0$.

(92) We will now assume that the square plane of 1 square foot area weighs 2lbs., and will consider the effect of the 25 Y shifts under the altered conditions.

To develop air resistance equal to 2lbs. per square foot of plane, we require a velocity of 35·35ft. per second, and the force of inertia of this weight at that velocity is 2·195lbs. greater than the simple weight of the plane, because the velocity is greater than the rate of acceleration per second of natural gravitation; and $\frac{2·195}{25} = 0·0878$lb. constant force on the Y path. The velocity of 11·95ft. per second will give this, and the relative fall in space is 2·22ft.; then

$$\frac{9·71}{2·22} = 4·37 \text{ ratio; or } \frac{25^2}{11·95^2} = \frac{625}{142·8} = 4·37.$$

In a free fall in space near earth, 1·1 second nearly would be required to give 35·35ft. velocity, and this time for that velocity, in space, would be the same for 1lb. as for 2lbs. The time of Y for ab is 1 second, and the velocities named are the rates per second, and we employ the 25ft. rate as our standard of comparison; hence the ratio last determined.

(93) If we now take a plane measuring 0·5ft. in the Y direction, and of 0·5 square foot area, and weighing 1lb., so as to represent one-half of the last-named plane of 2lbs. weight, and urge it forward so as to travel the Y distance ab in 1 second of time, we will have 50 Y shifts, and $\frac{2·195}{2} = 1·0975$lb. for the given half area; and $\frac{1·0975}{50} = 0·02195$lb. constant force on the Y path. The velocity required to give this on ½ sq. ft. is 9·4ft. per second; the fall in space due to this velocity is 1·37ft.; and

$$\frac{9·71}{1·37} = 7·088 \text{ ratio; or } \frac{625}{88·36} = 7·088$$

(94) Similarly with the square plane of 1 square foot area, when halved, and the halves separately moved from a to b. Thus, for 1lb. per square foot, the force of inertia is 0.776lb. as before determined, and $\frac{0·776}{2} = 0·388$lb. for the given half area, and $\frac{0·388}{50} = 0·0077$lb. constant

force on $\frac{1}{2}$ sq. ft. The velocity here required is about 6·55ft. per second; the fall in space 0·67ft.; and $\frac{9\cdot71}{0\cdot67} = 14\cdot5$ ratio; or $\frac{625}{43} = 14\cdot5$.

(95) Keeping the Y length 0·5ft. as in the last two cases, but making the lateral length 2ft., to get 1 square foot area, we have here 50 Y shifts in the distance ab; and for a weight of 1lb. have $\frac{0\cdot776}{50} = 0\cdot0155$lb. constant force on the Y path. But as we have twice the area of support in the distance ab by reason of the lateral extension, it is evident that a velocity which gives half the inertia force here named will suffice, $\frac{0\cdot0155}{2} = 0\cdot00775$lb. For this on 1 ft. sq. there is required a velocity of 5·1ft. per second, with a fall in space of 0·404ft.; and

$$\frac{9\cdot71}{0\cdot404} = 24 \text{ ratio; or } \frac{625}{26\cdot01} = 24\cdot0.$$

(96) Keeping the weight 1lb., and the area 1 square foot, but making the Y length only 0·33ft., with the lateral length 3ft., we get $\frac{25}{0\cdot33} = 75$ shifts of the plane in the distance ab for 1 second. Then $\frac{0\cdot7767}{75} = 0\cdot0103$lb. constant force of inertia in the Y path.

As the area of support however on the Y path ab is three times the area travelled over by the plane which measures $1\cdot0 \times 1\cdot0 = 1\cdot0$ sq. ft., a velocity which gives one-third of the above force will sustain the plane so as to make that one-third velocity uniform.

Then $\frac{0\cdot0103}{3} = 0\cdot00343$lb. force of inertia in the velocity required, which is at the rate of 3·7ft. per second. The gravitation fall in space to give this velocity is 0·2127ft., and

$$\frac{9\cdot71}{0\cdot2127} = 45\cdot6 \text{ ratio; or } \frac{625}{13\cdot69} = 45\cdot6.$$

We will now show the relative values of the ratios.

Ratios	9	4·37	7·088	14·5	24	45·6
Relative values...	1	·485	·787	1·63	2·66	5·0
		greater fall x.			less fall x.	

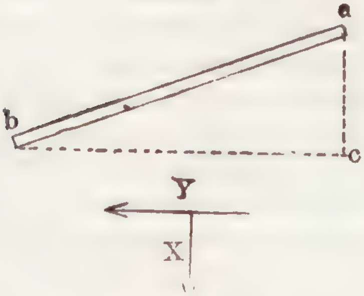
FLIGHT OF BIRDS.

Chapter XIII.

(97) We shall now consider the means by which the horizontal impulse Y can be produced with only the air to act upon as an abutment.

A plane in motion, with its surface perpendicular to the direction of motion, compresses the air beneath it, so that the air driven out of place escapes uniformly at the edges all round.

Fig. 19.

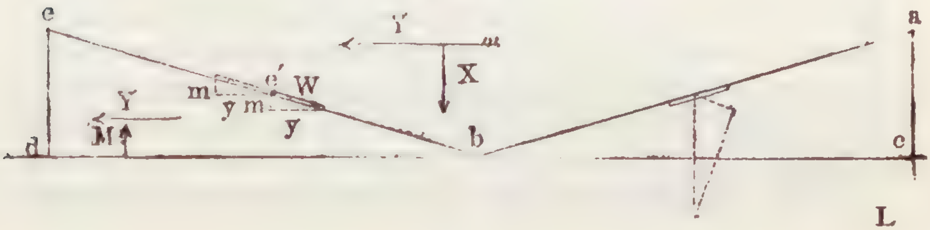


When the plane is inclined as ab (Fig. 19), and let fall earthward in the direction X, the tendency is for the air to roll up the face towards the upper edge a until the X velocity pressure develops inertia resistance in the air sufficient sensibly to sustain the weight of the plane; the plane then, thus supported, will tend to glide Y-ward; but upon the amount of edge resistance at b will depend the precise value of the velocity Y.

(98) In the first wing-strokes of a bird when commencing flight the air will give support equal only to the inertia resistance due to the velocity of displacement of the weight of air displaced.

A heavy bird, when free to choose the direction, rises against the wind when commencing flight, to get the sustaining force of the wind in aid of its own efforts; or lets itself drop slanting from a perch; or runs if on the ground, to bear its wings along the surface of a greater weight of air than it could put in motion by a wing-stroke if stationary; its wings when thus running labouring more than when fairly up in air with the Y velocity great.

Fig. 20.



(99) The impelling force is exerted in the down-stroke *a* to *b* (Fig. 20); and, though the wing has to rise from *b* to *e* to make the succeeding stroke, it rises free from resistance on its upper face.

Thus, let *Y* be the forward impulse given by the down-stroke, and *M* the motion of the rise to *e* for the succeeding down-stroke. Then *bd* represents the *Y* distance travelled by the wing *W* in the rise; *de* represents the rise; and—for convenience assuming the path of the rise to be straight—*Y* and *M* are represented proportionately by *y* and *m* for any point intermediate between *b* and *e*, the resultant of these two forces being in the path *be*; consequently, a particle of air caught at the leading edge *e'* of the wing, and deflected into line with the upper face, will not be further deflected, but will remain there until the rear edge of the wing has passed.

(100) Then, as regards the pressure on the lower face, we have the air supporting the wing against the *X* tendency to gravitate, as in the down-stroke; and as the wing by reason of its upward *M* motion is receding from the pressure; and as the force of inertia of the weight of body supported by the wing is very small at the start in a new direction *X*-ward; and further as the body has acquired *Y* momentum in the preceding down-stroke, we have the *X* tendency affecting very little in the time of the up-stroke.

(101) The impulse *Y*, however, owing to the air resistance on the surface perpendicular to the line of flight, will be weaker at *e* than when starting from *b*, and will consequently, when near *e*, make its shifts on to new air less quickly; and this slowness will allow the air near *e* to acquire motion from the pressure, and therefore yield *X*-ward; but as the impulse *Y* developed in say the half-time *cb*, is assumed to be equal to the force needed by the whole-time *cd*, the mean for *cd* is sufficient to carry the wing to *e* on a level with *a*.

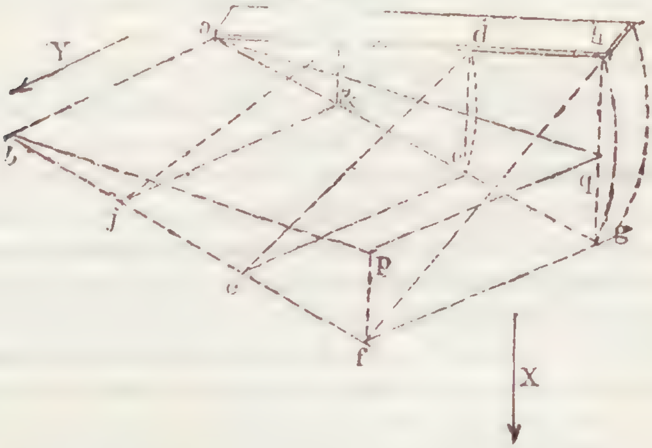
It is evident, however, that with the force developed in *cb* and acting by momentum only in *bd*, the body would have an upward tendency in *cb*, and a sinking tendency in *bd*, were the angles *bac* and *bed* equal. We must, therefore, suppose the time *bd* less than half the time of a full stroke, and the time *cb* consequently more than half; so that the impulse *Y* may develop its force on a long resultant *ab*, and the rise be on a short resultant *be*, in which, less time being allowed to rise the given height *de*, the tendency of the air to yield by the slowing of the *Y* impulse will be compensated by the quicker rate of the upward motion *M*.

(102) The bigger sea-birds, such as the albatross, are seldom seen

to flap their wings ; yet even in storms, they work their way to windward ; they do so, however, not in a direct line, but on a path which is a succession of varied curves, in which the weight of the bird is made to act against the force of the wind upon the outstretched narrow wings, so that the weight is floated. The resistance or force of the wind upon these outstretched wings is the same in kind as the resistance upon wings that strike the air ; and the difference lies only in the manner of employing it.

The albatross has long narrow wings, with the leverage great, measured from the joint at the body to the centre of pressure ; so that when in flight with its motion *Y*, equal to its own visible velocity plus the velocity of the opposing wind, were it obliged to strike the air in the manner of ordinary land-birds with broader and shorter wings, more power would be required than it possesses to maintain itself for days upon the wing in the face of the rudest gales. The albatross and other similarly winged birds, therefore, in place of making a wing-stroke to form a resultant *ab* (Fig. 20), let their whole weight descend with the plane of their wings at the mean angle *abc*, and thereby develop the force that will raise their whole weight on the rising resultant *be* ; and this is what an ordinary land-bird in part does, in launching out from a raised perch, to develop impulse *Y*.

Fig. 21.

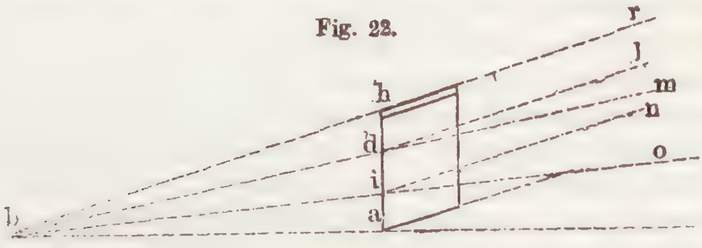


(103) In Fig. 21 let *ah* represent the wing plane in the act of making a down-stroke from *h* to *g*, and inclined to the *Y* direction at the angle *hfg* ; let *ab* be the *Y* distance for the time taken to make the stroke ; and let *i* and *d* be intermediate points in the plane.

Now it is evident that as the axis of oscillation in the body of the bird travels forward on the line *ab*, the wing-plane travelling with it and possessed of the same *Y* impulse, the tip *h*, which would descend to *g* were the axis stationary, will be made to descend on the resultant *hf*, and will reach *f* in the same time as *a* takes to reach *b*.

In like manner the points *d* and *i* will descend on their respective resultants *dc* and *ij*; and as the *Y* distances *ec* and *kj* are uniform with *ab*, it is obvious that if the inclination of the plane be uniformly the angle *hfg* from tip to axis, there will be air resistance on the upper face of the plane at the points *d* and *i* equal to the values of the respective angles *ldm* and *nio* in Fig. 22, in which figure *hba* = *hfg*, is the

Fig. 22.



assumed uniform angle of the plane from tip to axis; *dm* and *io* being the continuation of the respective resultants *db* and *ib* = *dc* and *ij* of Fig. 21; and *dl* and *in* being the lines of inclination uniformly parallel to *hb* or *hf*.

To do away with this resistance on the upper face, the plane may at all points have the inclination of the resultants for these points; that is, if the angle at the tip be *hba* (Fig. 22), the angles at *d* and *i* may be respectively *mba* and *oba*. But as we find the wing-feathers near the body are curved so as to lie fair with the form of the rounded body when the wings are closed, we have these feathers, where the wing-stroke is weakest, resisting the tendency of the weight to gravitate by opposing to it the upward pressure of the air on the lower face of their plane of inclination. In a bird-wing the flexibility enables it to assume the angles here indicated, according to the pressure caused by the wing-stroke.

(104) The velocity *X* may occur either in a direct fall earthward, or in a wing-stroke. In the latter case it may be regarded simply as taking place outside of the body that is being supported; the wings, sustained by the inertia of the body, developing upon their plane-surfaces in the stroke,—the motion of which is distinct from any other motion that the

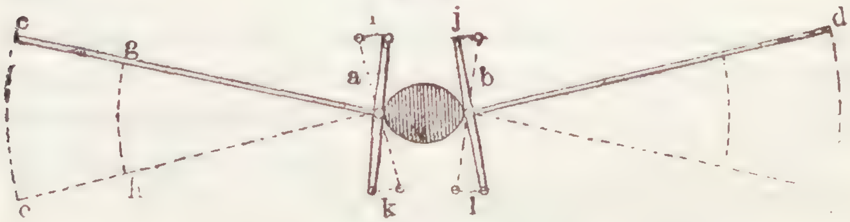
body may have,—the resistance that could otherwise be got only by body and wings descending together, supposing the wings merely outstretched

If the wing area to support a given weight be reduced, the air has less surface to act upon, and must be struck with higher velocity to increase the resistance per unit of surface, and to develop quicker Y velocity for quicker shifts on the Y path of support, to allow the extra weight of body no time to take effect in putting the supporting air in motion. And as quicker wing-strokes develop greater resistance on the wing-planes, so as to compensate for deficiency of wing area, it follows that, with a given area, a quicker stroke will at any time give greater Y velocity.

(105) The centre of pressure on the wing varies with the form; the distance of the axis of oscillation, or hinge, from the centre of pressure is the length of the leverage with which the pressure is acting.

In a bird the wing is worked by the alternate contraction and extension of the muscles massed about the shoulders, and it is hard to exhibit their action by lines merely; we will therefore treat the question as one of simple leverage.

Fig. 23.



In Fig. 23 let *ab* represent the body of the bird, looked at endwise; *ac* and *bd* the wings; *ce* and *df* the extent of stroke; *g* the centre of pressure; and *gh* the arc it describes in the stroke.

Now, to work the wings mechanically, let *ai* and *bj* be two short levers projected from the wing-planes at the axes *a* and *b*, so that each wing with its respective lever acts as a bent lever; then the length of the lever *ai* relatively to the length of wing leverage *ag* determines proportionately the force exerted at *i* to make the wing-stroke *gh*.

(106) When a bird with its wings oscillating rapidly is stationary on a perch, or in the first of a start to rise from off the ground, the developed force of inertia of the weight of wing in motion has to be expended, at the end of every up or down stroke, as a drag on something before the wing can be turned into the new direction.

Were the weight possessed of no area on which to develop sensible air pressure, the motive power would have to absorb the inertia force at every turn; but as a bird-wing is possessed of great area in proportion to it own weight, and as the air is compressed in the act of displacement, and by reason of its elasticity reacts upon the wing when the turn of the stroke is happening, the effort of the motive power has little to do beyond giving the simple weight of the wing the required velocity in the new direction; and the apparent difficulty that a heavy bird has in starting from the ground relates in its first slow Y velocity to the want of area of air surface in the Y direction.

J. A.

CONCLUDING REMARKS.

In our customary review of the work of the past year we cannot point to any special mark of definite success relating to the actual performance of flight; we are however cognizant of the fact that a great number of earnest workers are pursuing their favourite science with undiminished vigour, and with an amount of perseverance which must ultimately lead to a practical result. The papers now published show but a small per-centage of the amount of patient untiring work that is continually going on; and their perusal cannot fail to astonish any one who has not made aërial navigation a study.

In referring especially to Moy and Shill's "Aerial Steamer" we are not unmindful of the various attempts in course of progress by others; but we consider their machine, now nearly completed, to be the most determined attempt at solving the problem that has yet taken place, and therefore feel justified in calling special attention to it. The apparatus consists of two of their patent steam engines coupled together at right angles, with variable expansion gear capable of cutting off the steam at any portion of the stroke; the pistons are each 2in. in diameter, with a 2in. stroke, and working—it is said—up to about 4 horses' power. The total weight of the engines, generators, and lamps, all complete, is under 50lbs. The engines actuate four driving-

wheels 10in. diameter, and these wheels act by frictional gearing on the periphery of two aëroplane-wheels 6ft. in diameter. The aëroplane-wheels have each twelve light wooden planes fitted to them somewhat like screw propellers, but with this important difference, that the pitch is variable at every portion of the revolution. The action of these planes on the air is a perfect mechanical imitation of the direction of a bird's wing in the various positions that its surface assumes during the action of flight, giving, as it does, an upward and forward thrust continually, without any downward force from air on any of the aëroplanes.

The whole apparatus is placed upon wheels. It is intended to run it round a pole fixed in the centre of a circle of about 300ft. diameter. The theory is that the planes acting as an aërial screw will give motion to the ground-wheels, the friction upon which will decrease with each revolution of the planes, until it eventually leaves the earth, and will continue to traverse a circle in the air until steam is expended, when it will descend as gradually as it rose.

It may be anticipated that the difficulties attendant upon the attempt to make a model perform all that is expected of a machine of more practicable size will present obstacles here, only perhaps to be surmounted with time and perseverance.

The engine, for instance, which would be at perfect command in a machine designed for practical utility, must here be unapproachable whilst the aëroplane-wheels are revolving.

Again, the model must be proportionately very much heavier than a real machine, also possess self-action, and cannot have intelligent assistance to govern the angles at which the planes severally act.

It is certain, however, that should ascent be attained, the most rapid progress must ensue through absence of

friction upon the ground; and it is equally certain that if this desideratum be obtained with the model, the most encouraging results may be anticipated by the far more effective action of a large machine.

One of the main objects of the Society is to organize, and, as far as possible, direct the efforts of various inventors, and prevent experimental repetitions. Much waste of energy has taken place for want of a knowledge of what has already been done, and a loss both of time and labour from two persons attempting the same thing unkown to each other. This is a constant occurrence, and there is consequently much misdirected effort. In many cases plans are carried out which have been tried and failed half a century ago, and which would have been avoided if sufficient insight into the subject had been acquired in the first place by the perusal of our reports and the other published information to be obtained if duly applied for.

It is simply astonishing to hear of people trying year after year to drive elongated balloons, or gas-bags somewhat of the form of a German sausage, with a car underneath it, and a *screw* of course. This is generally the first conceived project of any one commencing to think upon the navigation of the air, and each one fancies himself the happy possessor of the secret. Yet what a very small amount of science is necessary in order to show its fallacy. A balloon simply floating in the air with its car and occupants is in a state of stable equilibrium. The centre of *displacement* of the whole mass is less than half-way down between the top of the balloon and the car, while the centre of *gravity* is very near the car; but of course *both* are in the same vertical line, and the whole floats perfectly quiescent and upright. The horizontally elongated balloon is much more difficult of management even when only floating; the centres of displacement and of gravity are brought much nearer together, and,

from want of stiffness in the gas-holder—and experimenters should bear this in mind—the centre of displacement in a sausage-shaped balloon is more easily disturbed than in any other form. Take a spirit level and notice now difficult it is to keep the bubble in the centre, and this is in a rigid substance—glass. But we have seen sausage-shaped balloons turning up on end very curiously, and the gas swaying from one extremity to the other in a very uncomfortable manner. And if all this takes place when merely floating, how much more so when an effort is made to *drive*, not in a line with the centre of displacement, but, as it is in all cases attempted, very near the *centre of gravity?* It is something like a boy tying his string to the bottom of his kite instead of to the line of the centre of resistance. We make these remarks in order to illustrate one of the sources of waste of energy.

It will be observed that several trials have been made by different experimenters relative to the hoisting and lifting power of screws, the latter of large size, acting vertically and supported and balanced on an arrangement suitable for ascertaining the actual lifting power in pounds beyond the equipoise that a man is capable of raising by his own muscular force, the results have varied considerably according to the perfection of the mechanism; but it has been proved that a man can sustain a weight by this means equal to one-third that of his own body, and as the result seems to depend on the construction and its most appropriate form, some hopes may be entertained that, by improvements, this lifting force may be far exceeded. Though it is not to be expected that a man can ever entirely lift himself in air by such a machine, yet the experiments are important and interesting as bearing on the question of the use of inclined vanes rotating like a screw, as a means of propulsion only. With all the tried experiments that have come to our knowledge, we can safely say that up to the present time no man has yet adapted

himself to a machine that has fulfilled what we have learned to consider the true law and principle of flight in order to test the lifting force during a rapid horizontal course. It is under this condition only that the substratum of air passed over—on account of the enormous weight of air impinging on the aëroplane in a brief time—cannot be deflected to any great extent by the comparatively trifling weight of the machine, which, under these conditions, finds an unyielding support. There are very great difficulties in the way of trying the necessary preliminary experiments, which can only be carried out satisfactorily in an open car attached to a railway train at a high speed.

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THE FOLLOWING

SPECIFICATIONS OF PATENTS.

No.	*Date.*	*Subject.*	*Patentee.*
	1873.		
245	Jan. 31.	Aërial Tramways	Henry Jno. Puckle Richard Fenelly.
2776	Aug. 21.	Improvements in the Construction of Balloons and other Aërial Bodies, and in the Means of Navigating them so as to control the direction in which they shall move or travel	Margaret Martin.
3309	Oct. 11.	Improved Method of, and Appliances for, determining the course or direction of bodies in air and water	Albert Fleury.
4255	Dec. 27.	Aëronautical Apparatus	Jean Chas. Gaveau
4279	Dec. 30.	Improvements in Apparatus for raising, forcing, and exhausting Water, Air, or other Fluid; also for lifting, directing, and guiding Balloons and Flying Machines	John Collis Browne.

BOOKS, PAMPHLETS, &c., RECEIVED.

An Account of the First Aërial Voyage in England, in a Series of Letters to his Guardian, Chevalier Gherardo Compagni, by VINCENT LUNARDI, Esq., Secretary to the Neapolitan Ambassador, with his Autograph; published in 1784.—Presented by Mr. H. S. RICHARDSON.

Autograph Letter of — Cocking, who lost his life by descending in a Parachute.—Presented by Mr. H. S. RICHARDSON.

Smithsonian Reports, 3 vols.—Presented by the SMITHSONIAN INSTITUTION, WASHINGTON.

Screw Blades.—Presented by H. C. LINFIELD, Esq.

India Trigonometrical Survey.—Presented by the INDIA OFFICE.

L'Aëronaute, Monthly Reports of the Aëronautical Society of France.—Presented by Mons. le Docteur DE VILLENEUVE.

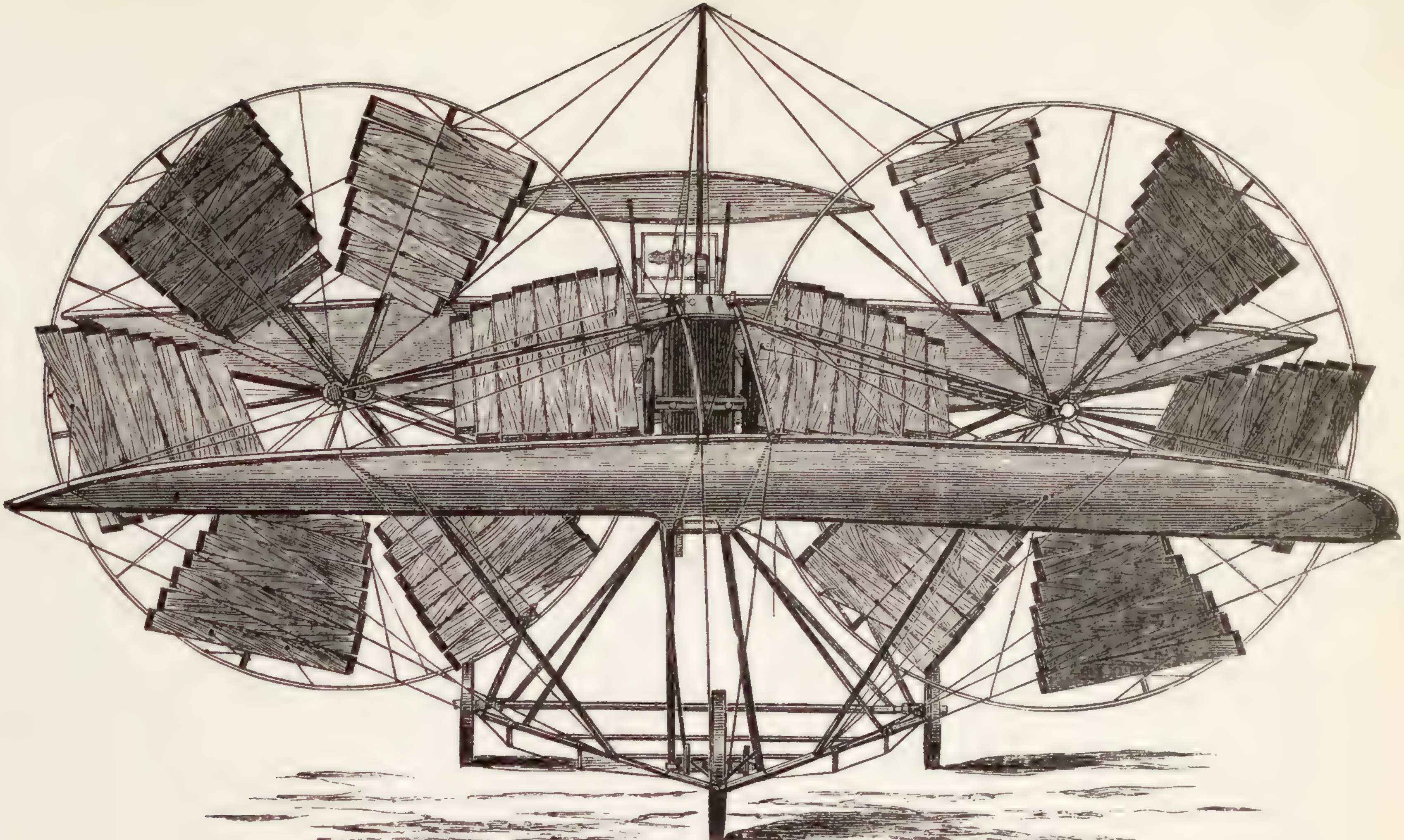
Il Problema dell' Aëronautica Lettera del Prof. PASQUALE CORDEMONS al Signor ALESSANDRO FERRETTI.—Presented by the AUTHOR.

Problema dell' Aëronautica, Soluzione del Dott. PASQUALE CORDENONS, Professore di Matematica Nel Ragio Liceo di Rovigo.—Presented by the AUTHOR.

Supposizioni di Nautica Eterea per VOLANTE ALESSANDRO.—Presented by the AUTHOR.

Daily Bulletin of the Signal Service, U.S.A., with the Synopses, Probabilities, and Facts, Sept., 1872.—Presented by the WAR OFFICE, U.S.A.

INDEX.—"WINGS FOR MAN."



THOMAS MOY'S AERIAL STEAMER.

Ninth Annual Report

OF THE

AËRONAUTICAL SOCIETY

OF

GREAT BRITAIN.

FOR THE YEAR 1874.

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Ladies may become Associates upon the same terms.

Ninth Annual Report

OF THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN,

FOR THE YEAR 1874.

Containing an Account of the Proceedings, and a Selection from the Papers and Communications received by the Society during the year, with concluding Remarks upon the present state of the Science.

THE Annual General Meeting of Members of this Society was held in the Rooms of the Society of Arts, on Thursday Evening, the 15th of May, 1874. Mr. Browning, F.R.A.S., presided.

Mr. FRED. W. BREAREY, the Secretary, read the minutes of the previous meeting, which were duly confirmed.

Mr. BREAREY stated that since last Meeting a gentleman in France had constructed a model which would fly by the wing alone; it weighed about three ounces, measured 32 inches from tip to tip, and would fly 50 feet. They had also been constructing in New York, an apparatus with superposed aëroplanes 10 feet in diameter, made of sheet-iron, worked by an engine of 15 horse power, and weighing three tons. It was 58 feet high and carried a car. It had been exhibited at so much a head but had not yet been tried. Perhaps he ought to caution the Members because he believed it was intended to bring it over and exhibit it here. It was of no use from the point of view taken by that Society, because it

would be impossible to raise a machine of that weight with 15-horse power.

The CHAIRMAN: Mr. Moy will first address the Meeting on the experiments conducted at Messrs. Penn's, Greenwich, for the furtherance of the work of the Society.

Mr. MOY said they would remember that some time ago some very interesting and valuable experiments were made at Messrs. Penn's factory, at Greenwich. The results of those experiments went far beyond his expectations in favour of aërial navigation, and gave upward pressures at small angles which were not expected from any existing theory. From the data furnished by these experiments, Mr. Moy had constructed a diagram of curves for pressures at different angles, and at speeds varying from 10 to 40 miles an hour. If they looked at the diagram they would see that from a perpendicular position down to 45 degrees there was not much to be gathered. At 45 degrees the upward pressures and forward resistances were equal; but when they came to fine angles, they would observe that the curves indicating upward pressure showed a remarkable fulness while the curves of forward resistances were very small.

As an instance he would take from the tables the speed to be 40 miles an hour: at ten degrees the upward pressure was 2·84lbs. per square foot, while the resistance was only 0·4lb. per square foot. With regard to onward and upward motion they would find that these curves indicated a lifting power which it was never before expected to get. These curves derived from actual experiment explained to his mind most thoroughly how it was that large birds such as rooks and pigeons were able to fly with the wings in a rigid state over considerable distances without any apparent exertion, and they confirmed and explained what they were continually observing in nature.

Mr. Moy had not yet attempted to construct a formula from these experiments, and perhaps some clever geometrician or mathematician would do so at a future time, and thus demonstrate the true law of air resistance. He trusted that members would turn their attention to the results obtained from these experiments, for he looked upon them as very essential to those who were making experiments in flying. He had no doubt that less horse power would be required than had frequently been anticipated.

Mr. Moy next stated that he understood that some time ago Mr. Coxwell had promised to assist in trying experiments with a balloon with a vertical screw, in order that the balloon might be raised or lowered at pleasure, by manual or other power. Mr. Coxwell's ill-health unfortunately prevented him carrying out these experiments.

Mr. Brearey: That was 18 months ago.

Mr. Moy said that Mr. Glaisher had lately had a conversation with him as to carrying out these experiments, and which Mr. Brearey was also anxious to see carrried out. He (Mr. Moy) proposed a plan composed of two hoops, the inner hoop being suspended to the balloon in the usual position, and the second hoop being large enough to surround the first hoop and revolve on rollers with a driving wheel and cranked axle, this axle to be actuated by two of the occupants of the car by means of treadles. He calculated 40 revolutions per minute of the axle would be a reasonable speed. Outside the outer hoop a number of aëroplanes would be fixed; the angles of which would be capable of alteration for experiment. By these means Mr. Moy thought that a lifting power of from 30 to 40 pounds could be ensured, and ascent and descent to that extent would be obtained without throwing out ballast or letting out gas. It would form an excellent summer afternoon's excursion to be able to float with the wind, and to ascend and descend at pleasure. He under-

stood the Society were going to purchase a balloon which would afford the means of furthering the object in view. A third subject which he wished to speak upon, was their own aërial machine. They had not got on so fast as they had hoped. Various difficulties had arisen which had to be mastered one by one as they cropped up. As their machine was only a working model the aëroplanes required to be made of some light material. Some time ago he was trying to get thin steel rolled and corrugated but he was unable to get anything suitable. He next tried a fine kind of brown holland for their wing sails, but they found this would not take the curves kindly but got into wrinkles. He was now making use of thin pine laths nicely planed and carefully fitted so as to form a sectional screw surface, and he thought they would answer very well, and would form the nearest approach to a perfect screw that could be obtained. He hoped that in a short time they would be able to try further experiments.

The Chairman said that before he entered upon any remarks he would ask visitors who were present to consider themselves as members of the Society for the time being, and would invite them to take part in the discussion. The Society would be pleased to hear them. From the time when he was twelve years of age he had watched the flight of birds, and he had noticed that they never set their wings at any large angle except for the purpose of stopping themselves. The observations that had been made and the tables would be very valuable to the Society, and he thought they ought to have the tables printed. It had recently come to his knowledge that these experiments at Messrs. Penns' would be very useful to engineers. Doubts had been cast on those adduced by Mr. Nunn, but when he heard of these experiments of Mr. Wenham. at Messrs. Penns', he got the tables examined and he found they gave the same results as he had arrived at. Opposition was of course then at

an end. As regards the suggestion that had been made, and which was now being carried out, that a balloon should be fitted with revolving apparatus and a power which could be exerted by two men, should be tried; their friend Mr. Brooke had given some valuable advice. It was not so well known as it should be that Mr. Charles Brooke had done a great deal of work in his lifetime for which the scientific public were much indebted to him; therefore the earliest opportunities should be taken to give publicity to his valuable suggestions. The first suggestion was that the balloon should have weights placed in the car, that would exactly keep it on the ground. These weights should more than counterbalance the balloon, but as little as possible. Then he (the Chairman) suggested that there would be a difficulty in finding out what the effect of revolving fans would be, because the balloon would be in the position of a captive balloon, and would have to contend with the action of the wind upon the fans, which would tend to raise the balloon. The first thing required was that they should ask the Directors of the Crystal Palace to allow the experiment to be tried there, but that might be answered by the objection entertained to keeping so large an amount of gas within the building. Mr. Wenham bore him out in saying that they would have great difficulty in getting satisfactory results from a balloon used in that way, because when a balloon travelled in the air it was affected by the wind, while as long as it remained on the ground it might be considered as a captive balloon, and therefore the planes would depend upon the power of the wind passing through them. With regard to Mr. Brooke's suggestion that gentleman thought it desirable to dispense with the balloon altogether. The balloon was a resisting force. Let them suppose instead of having the balloon attached to this rotating fan, which was being worked by two men, they had a rope attached to the centre of a fan and carried it over a pulley; it was

quite evident that the experiment could be made quite as well by these means as by means of a balloon. They would get rid of the disturbing force of wind on a large surface, and no one would have any objection to allow the experiment to be tried in a closed building. The effects might be tested by self-registering apparatus, of which, with the permission of the Council, he purposed making a present to the Society. He would like further to remark that it seemed to be the idea of the Council that this was simply an experiment to see what effect could be obtained by rotating planes in this manner, and was not made with any idea of balloon propulsion except in the way indicated. He would now suggest that a vote of thanks should be given to Mr. Moy for the paper and diagrams; and he should propose that the tables which Mr. Moy had promised should be printed in the Society's proceedings.

Mr. Brearey said he had a short communication to read from Mr. Artingstall, of Manchester, as follows—

"You will no doubt remember that I have frequently intimated in writing to you that I did not believe that true flight is accomplished by waftage, or windlike action, as a ship is driven, a windmill turned, a boy's kite supported, or as an aërial screw acts; but that the pulsations or waves of air play an important part in flight; yet they cannot be easily utilized artificially as it depends upon the dexterity of action in the wing, if I may so speak, to 'catch them.' This, the feel of a bird's or bat's wings easily and naturally accomplishes. The following *may perhaps* bear on the subject:—

"I once mentioned to you in a letter a curious effect I produced by striking the air with the edge of the wing:—The following is an improvement on that experiment.

Fig. 1.

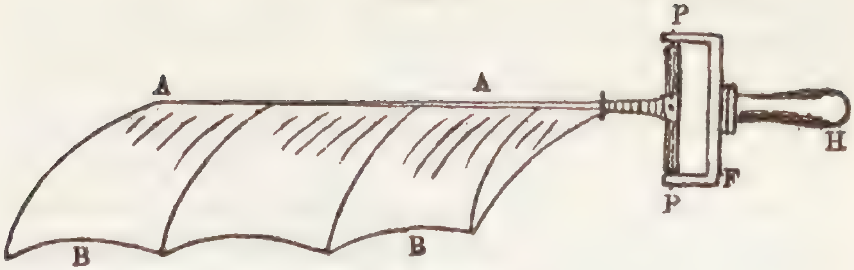


Fig. 2.

"Let *AA*, Fig. 1, be the thick edge of an artificial wing; *BB* the thin edge; *H* the handle. The stem of the wing is attached to the centre *PP* that turns to the iron fork *F*. To operate with this instrument, take hold of the handle *H*, and use the wing as though it was an axe or chopper, and by a *slow* stroke, endeavour to cut the air with the thin edge of the wing. The elastic materials of which it is made, will allow the plane of the wing to turn either one way or the other, and the oblique action will drive it as far as it can go sideways (*see* Fig. 2), on the principle of oblique surfaces acting in the air. This is easily conceived; but the following is not so, if you

hold the wing steadily by the handle H (Fig. 1), and the stem horizontal, and in a line with the handle, allowing the plane of the wing to hang perpendicularly as in Fig. 1. All being now prepared, endeavour to cut the air as before by a *quick* stroke downwards. If you hit upon the right position, the wing no longer *appears* to obey the ordinary laws of resistance of air but gives a powerful pulsation, and instead of being driven according to its obliquity positively goes in the opposite direction. This experiment requires a little practice to work it well, and to catch the exact point of pulsation.

"I hope some Member of the Aëronautical Society will solve the problem ; perhaps it is very easy after all, but *I* cannot see the solution at present.

"I *may* attach too much importance to the pulsatory action of air in flight, but that some such action takes place I have *proved by experiment*, to my own satisfaction.

"The resistance of air being as the 'squares of the velocities' is a mere school or college absurdity. The resistance depends upon the form of surface, the mode or condition of impact, &c. The effect of military projectiles, and the theory of their motions is much nearer the truth when applied to the impact of air, or what is the same thing, impact of sufaces against air."

The writer said he would have sent a model but it was only in a rough and unfinished state.

The Chairman remarked that it was a great pity the gentleman had any scruples about sending his model, because even engineers rarely made their trials with materials of finished workmanship. The model would have enabled them to try an experiment and might have brought forth remarks on the subject. As it was he would ask them to pass a vote of thanks to the author of the paper.

Mr. Brooke said he would only offer one remark as to the

resistance being in proportion to the square of the velocity. It was supposed that the particles resisting the motion as soon as they came in contact were annihilated, and only fresh particles came in contact with the moving body. This was at variance with what really must occur. The particles were compressed and had all to get out of the way sideways. The simple resistance and velocity were not sufficiently taken into account in the experiments at Penn's. It was supposed that whether the plane was placed so (referring to diagram) at 45 degrees, or so (at a less angle), the resistance would be the same in both cases, whereas it was found to be practically different. Suppose one was placed at 45 degrees to a current of air, it was clear that the air must pass along the surface of the plane. The resistance of a plane placed at the same angle, but in another position, would be different, whereas, according to mere mathematical law, they ought to be exactly alike.

A MEMBER: How large was your current of air, and what size was the disc?

Mr. WENHAM: I think the largest was two square feet.

The CHAIRMAN: Six inches by two feet.

The MEMBER: What size was the current of air?

Mr. BROOKE: It was passed through a shaft 18 inches wide. The object was about two feet in front of the current of air, but this did not make much difference. Of course the object must not be too far from the current of air.

The CHAIRMAN said it would perhaps be agreeable to the Meeting that the results of the experiments should be read.

The results were read accordingly.

Mr. WENHAM said that the law of the square of the velocity applied to water, and that a very different law applied to an elastic medium like air, where the resistance took a very different form to what it would in water. The law in regard to water had been approximately ascertained, and he thought,

with regard to the air, the law, when enquired into, would be equally recognizable.

Mr. D. S. Brown read the following paper :—

THE AËRO-BI-PLANE,

OR

FIRST STEPS TO FLIGHT.

In a former paper on the aëroplane, I described how its stability could be increased by employing two planes for support, one placed before the other. By experiments which I have since made, I find this improvement can be carried still further by constructing the anterior edges, or frames, of the planes rigid, and the other parts yielding or elastic. The modification admits too of the car or load being placed between the planes, and without any other force than gravity the bi-plane when elevated will proceed for a considerable distance in an oblique direction until it reaches the ground. Indeed, in this simple form, it may be aptly termed a progressive parachute. But with the aid of manual or other power the distance to which it could be propelled horizontally would be more or less increased, or the machine maintained permanently in the air. Nor are these the only advantages which it possesses. All shaking is either prevented or utilized. For if it be of a pitching kind the planes act as fish-tail propellers; or, if of a rolling kind, as wing propellers.

The apparatus is also able to descend very lightly, which is accomplished by bringing it, or the planes by means of the rudder, suddenly into a large angle with the horizon, which at once stops all motion precisely as done by a bird when it alights.

Fig. 2.

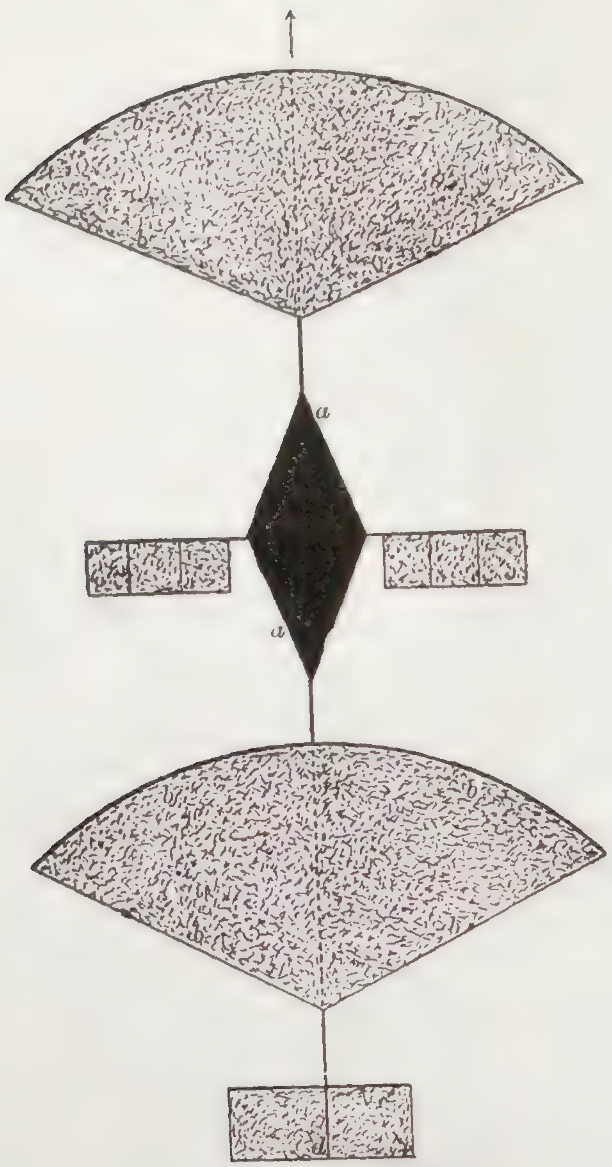
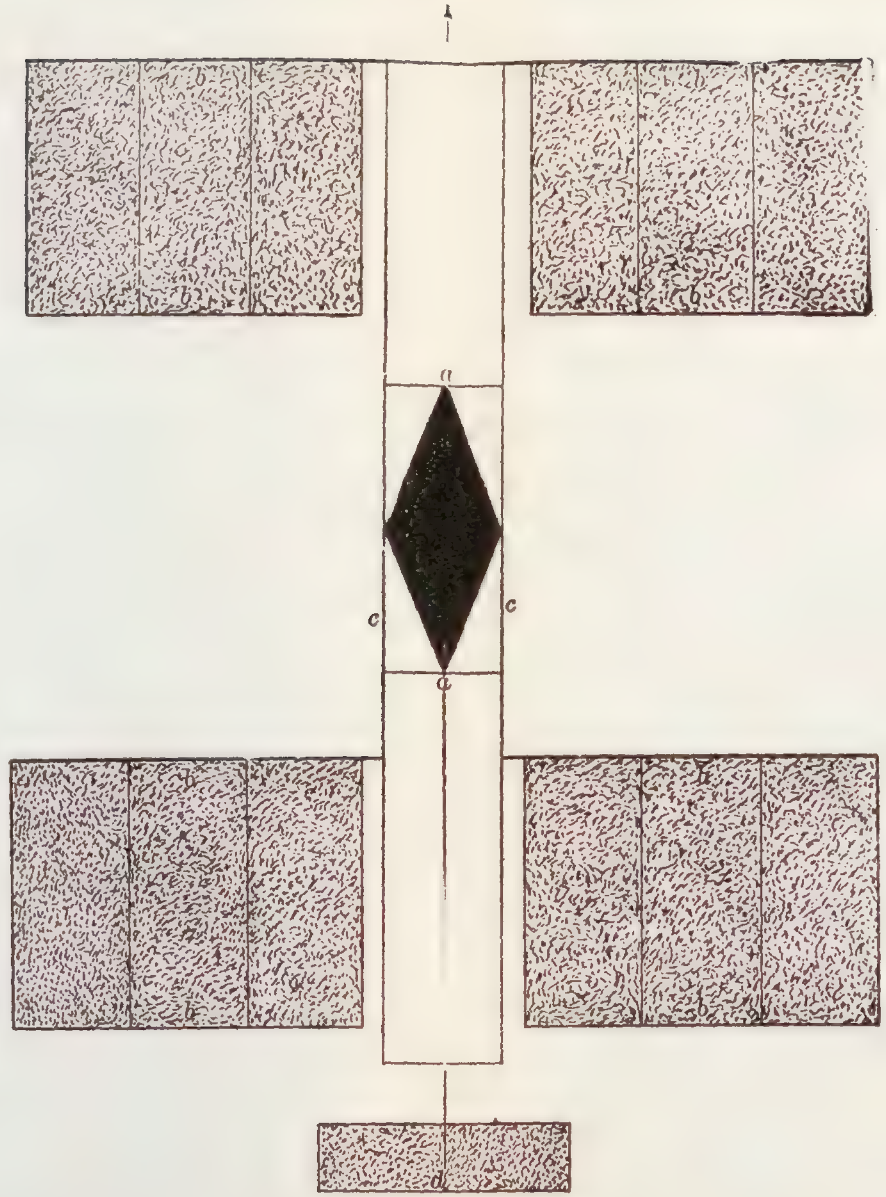


Fig. 1.



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Notwithstanding the elasticity of the planes they will be kept in their proper position by the air when moving through it, and all strain upon them prevented, affording at the same time ample support. As regards propulsion, a wing motion, which must necessarily be a slow one, can be given to either one or both of the planes, or a small pair of propelling wings may be attached to the car.

A man in a recumbent position offers very little resistance to the air and yet can exert great force with his feet in working a bellows engine, or they may be moved by a steam engine in a very simple way if the shafts or shoulders of the wings terminate in forks or prongs. A bar fixed to the top of a vertical piston rod and passing crosswise through the forks would then elevate and depress them at every stroke. The revolution of a crank in the forks turned by a spring would have the same effect.

Fig. 1 represents the improved bi-plane, *a* being the car, *bb* the planes which are divided at their centres in order to allow the rods or poles *cc*, which connect them, to pass to the forward edge of the anterior plane, and also to extend beyond the limits of the posterior one to support in a good position the rudder *d* consisting of two planes, one set vertically and the other horizontally.

Fig. 2 shows a modification of the apparatus, the anterior frames of both planes being curved to diminish the resistance of the air, and the posterior parts may consist of cord. Unlike a ship or wheel carriage, the bi-plane can only be supported by the air when it is in motion, as illustrated by a slate thrown upon smooth water, which is then sustained when moving horizontally without any inclination of its surface. It is therefore necessary that the apparatus should start with an initial motion, which may be given by an india-rubber rope fastened at one end to a post, and at the other, by means of a ring, to

a vertical bolt inserted in the under part of the bi-plane. On stretching the india-rubber by drawing the machine backwards, it will afterwards spring forwards with any required velocity, at the same time releasing itself from the rope as the ring falls from the bolt when the rope slackens. With respect to the proportion of weight to surface, that will depend upon the velocity.

I may mention, however, that in the experiments made by Sir George Cayley, a square foot was found to support more than 2lbs., and the Australian crane, a very large bird, weighing upwards of 20lbs., and one of the best flyers, is loaded to the extent of about 2½lbs. to the foot; whilst the crow only carries 1lb. to the same surface, and smaller birds and insects much less weight still in proportion.

As the advantages to be derived from the perfection of aërial navigation are not sufficiently appreciated or understood, I will, in conclusion, briefly state them. It will combine the independence of a private carriage with the speed of an express train, and a person will often be able to arrive at the end of his journey by the time it now takes to start from the nearest railway station. Aided however by favourable aërial currents the voyage would sometimes be made in an incredibly short time, as now done by birds of passage. It would also be in a straight line and free from all obstruction. The motion would be of the most agreeable and least fatiguing kind, resembling skating, and in warm weather the aëronaut would be able to choose what temperature to travel in, which would depend upon the elevation, whilst the expansive and ever changing view would be unrivalled.

It is very desirable that the attention of country gentlemen should be attracted more to the subject, because not only have they the means and leisure, but, what is of the greatest importance, ample facilities in their parks for trying the experiments

on a large scale, by which alone mechanical flight can be rendered a reality. I have shown how stability can be combined with progressive motion and guidance; how the planes can be constructed so that the air will remove the strain from them; and how a start and descent can be made with safety. I have also devoted much attention to motors and find that abundant resources exist for making them light should extraordinary lightness prove to be essential.

In the course of general remarks, with which the reading, illustrations, and experiments were interspersed, he said that before he began his experiments he thought that power was the great desideratum, but he soon found that the question of stability was, if anything, of still more importance not only to ensure safety but also for economizing the power itself. He had now constructed a plane which, when thrown into the air at whatever angle, always returned to its normal position. Most of them would recollect the experiments of Mr. Henson and those of Mr. Stringfellow, who, at the Exhibition of 1868, contributed a model of a flying-machine. He had a superabundance of power, and yet could not release it in the air, because he had not overcome this difficulty, Mr. Brown thought, however, that he had done so by using two planes, which gave a stability somewhat analagous to what is obtained by supporting a beam at both ends instead of only at its centre. A weight placed between the planes constrained them to assume a horizontal position like a well-ballasted ship, and without the aid of a rudder. Until this had been accomplished, it could not be said that they had even laid the foundation-stone of their art, and it would be for them to say how far he had succeeded. This (a slight plane of light wood and paper) was the first form. He would set off with a pitch. That pitch would not be made in its normal position, but it would return to its

natural position if there was room for it to do so. The next improvement was to make the machine elastic, which must also be done so far as possible in order to perfect its safety. The machine, he might say, got no support until actually in motion.

Mr. Brown launched several planes of different dimensions. All showed perfect stability, and, save one or two, floated in the air in a horizontal position across the room, a distance of between twenty and thirty feet, and, apparently, in some instances could have gone further without falling had not the walls intervened. One he suddenly pressed downwards in a perpendicular direction by striking it with a stick when in the air: this caused it to dart forward with great velocity in a horizontal course. Mr. Brown considered this an illustration of true flight, as the planes were only inclined the moment he struck the connecting-rod. During the flight they recovered their horizontal position, and offered no resistance to the air.

Mr. Brown added that it was desirable that the attention of country gentlemen should be attracted to this subject from time to time, because they had not only means and leisure, but they had the opportunity of making experiments on a large scale in their parks.

Mr. Brown next exhibited a shallow boiler made of very thin metal, containing a little water, for the purpose of inflating a tiny elastic balloon fixed on the boiler. The object was to exhibit powers of distension and contraction by the application and removal of heat in order to imitate wing motion, which on a large scale must be slow on account of the length and obliquity of the stroke. This kind of engine required no valves, and the principle admitted of its being worked as economically when the pressure of the steam was only 1 pound to the inch as when it was 15, which of course was of immense importance as regards its lightness.

Mr. MOY: What pressure do you work at?

Mr. BROWN: This is a mere experiment.

Mr. MOY: What will it bear?

Mr. BROWN: I have here a small aluminium tube 9 inches long and one in diameter. No doubt it will bear an internal pressure of 100lbs. Aluminium is four times as light as silver, and it can be rolled much thinner than copper, without cracking like that metal. Unfortunately it cannot be soldered, but I have succeeded in uniting very thin pieces weighing half an ounce to the square foot, by sewing them together and placing a leather or india-rubber washer between the united parts. This tube appears perfectly made, without a joint, but how I do not know. It weighs three-quarters of an ounce.

Mr. BREAREY: The boiler is now full of water.

Mr. BROWN: No, there is only a wineglassful.

Heat was applied, and the bulb slowly expanded, and on its withdrawal, and the application of a cold sponge, contracted.

Mr. BROWN said he was at one time an advocate for steam flying; now he thought they ought to try manual power first for many reasons.

(Model Exhibited.)

Mr. MOY asked the price of the aluminium for constructing the boiler.

Mr. BROWN said about 8s. per oz.

Mr. WENHAM said it was about 5s. in bulk.

The CHAIRMAN thought it could be got at about 4s. when taken in quantities. It was quite evident that Mr. Brown had proved that the bi-plane had a tendency to keep its own position, and this he thought was a matter of some value. If Mr. Brown gave to it a tendency to an upward direction a long flight might be obtained. They ought not to pass over Mr. Brown's liberality with respect to only patenting his inventions provisionally. Even experiments begun on false

hypotheses might lead to valuable results, just as the search for the philosopher's stone led alchemists to better discoveries.

In reply to a Member, Mr. Brown said he had not given up the idea of the use of steam in the air, only he gave the preference to manual power for a commencement, and if that failed they could fall back upon steam. On a plane, without any inclination, he believed man had power to fly, but with inclination he doubted whether, even with a steam engine, they could accomplish it.

Mr. Moy considered Mr. Brown's proposal for preventing the plane from falling by giving it a partly upward course was equivalent to inclining it. In fact it was a distinction without a difference.

Mr. Brown thought it would be found different in practice, give greater stability, and offer less resistance. But the idea did not originate with him.

A Member asked if there had been any experiments made with electricity to give motive power?

The Chairman thought not, and expressed his belief that engines required for that purpose would be too heavy to use in the air.

The Member: Has any one tried atmospherical electricity —getting motive power out of the atmosphere.

The Chairman: No, I have not heard so.

Mr. Brown said an example might be drawn from the bicycle where manual power was more effective than steam.

A Member: Has there been any experiment with the view of getting gas out of the atmosphere?

Mr. Wenham: I am afraid all such schemes must fail. We never could get aërial machines lighter than one ton per horse power with either gas or air engines.

Mr. Brown, replying to questions, said it was difficult to incline one plane and keep it in its position. There must be two planes if they inclined them.

The CHAIRMAN said there was another paper, but time would not permit them to read it.

Mr. SÉNÉCAL remarked that elasticity, so well illustrated in birds, insects, and fishes, does not seem to be be applied in apparatuses intended for aërial locomotion.

.The planes of a machine, should be composed of materials well known for their elastic properties,such as Indiarubber, steel, &c.

By constructing a machine, structurally and mechanically on the laws of elasticity, you will reduce it to at least a tenth part of the original weight; in the same proportion, the power for progressive motion will be considerably increased, while the main or motive power will be reduced to a mere trifle.

He pointed out in Mr. Brown's model that the planes should be elastic, not only in the line of motion but also at right angles to that line. The resultant forces will then be a forward motion of the whole.

A weighty machine may be very effective at high speed or in a strong wind; but I think you will require speeial apparatus for coming down in a calm.

A machine ought to be constructed as light and elastic consistently with strength to carry a weight (which will be the equivalent to work done), the whole will be a store for power, which can be effectively developed as before mentioned. Elasticity is a force that can give very powerful effects, and under suitable conditions is capable of being developed almost indefinitely. An elastic ball will rise according to the force with which it was thrown on the ground, and yet it is far from being a proper shape for its production. It is by elasticity, so well directed, that birds and insects steer themselves with such dexterity and precision.

The CHAIRMAN remarked that it was quite true that under some circumstances elasticity in the apparatus might

serve as a store of power, but first of all the power must be communicated before it could be worked. There is considerable elasticity in the wings of birds, and if a wave of air came in front of them they availed themselves of it in the most economical manner.

A vote of thanks was given to the Chairman.

The Meeting then closed.

The following paper by Mr. James Armour, C.E., was taken as read :—

RESISTANCE TO FALLING PLANES

ON A

PATH OF TRANSLATION.

I.

In a body propelled by planes in motion, the centre of gravity of the weight of body is assumed to be sustained on a uniformly level path, to be sustained in the manner of a weight at rest, without momentum in the direction of support; whereas the propelling planes will move on an undulating air path.

The resistance of the air in the case of bodies moving in it with moderate velocity, has been experimentally found to vary, roughly with V^2; consequently, as the density of air is proportional to the pressure, we have the air which is driven with velocity $20{\cdot}5^2$, of about one-fourth the density of air driven with velocity 41^2; and as the expansive reaction of the pressure due to density, is weight of resistance to be overcome, we have the greater or 41 feet velocity carrying the fourfold weight of resistance through twice the actual space in a given time, so that the ratio of work done in a given time is here 8 to 1.

Further, as the resistance to the wing plane in motion is for the support of the body, it must be equal to the force of inertia that the weight of body would develop by free motion in a given time.

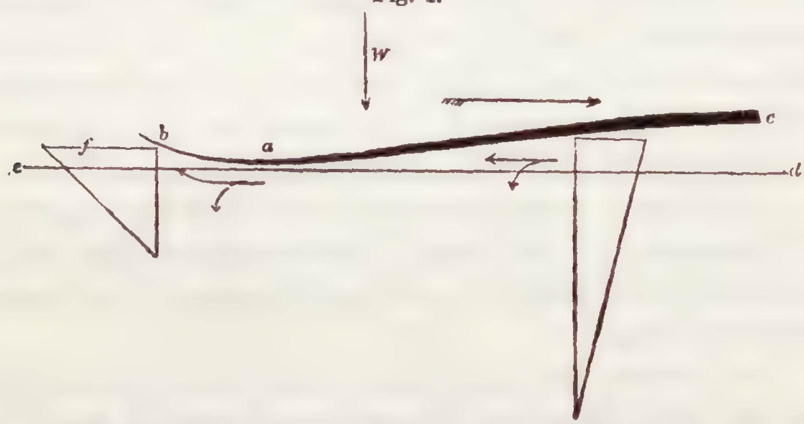
In the case of rotating planes, were they flat and rigid, the propelling power would have to be performed wholly by the inclined face of the plane; and as at the flatter angles for quick motion of translation, the horizontal component for im-

pulsion, upon which the maintenance of the motion of translation depends, would be insufficient, it is evident that some force distinct from this horizontal component of the flat rigid face is needed; and in the following brief observations we hope to show how this force may be obtained.

In a bird's wing the front or leading edge is stiff, and the rear edge is flexible; flexibility occurring also in the direction of the length from body joint to tip.

In the case of rotating planes, if the wing plane be flat and rigid, the expansive reaction of the air that has undergone compression takes place when the plane has passed, and is therefore so much power let go with, say only half its capability utilized. Whereas if the plane be possessed of flexibility at the rear edge, the force of compression that has taken the time *ac* Fig. 1, to develop, reacts in the time *ab* on the flexible rear membrane of the plane; and as this reaction in less time represents greater energy in the act of expansion than in the act of compression, we have the angle of the curve at *b* greater than at *c*; and consequently have the impulsive force *f* greater than the unit resistance on the face *ca*.

Fig. 1.



The gravity of the weight supported performs the compression, and in the elastic reaction on the rear edge of the plane,

we have the energy of the compression reacting to impel the loaded plane along the path of translation.

Assuming that the rotating planes start with the weight they have to sustain from a state of rest or zero, and without the support of the ground to run upon, the duty of overcoming the force of inertia in the whole weight for a given sustaining velocity of translation would devolve upon the planes wholly; and as from a certain distance from the start the velocity of translation must be slow, and the path of support for a given time correspondingly short, the velocity of rotation to give force of stroke sufficient would have to be correspondingly great if the path of flight be horizontal.

At the point however where the sustaining velocity of translation is reached and continued at uniform rate, we have the force of inertia of the weight developed for that velocity, and no longer exacting horizontal impulse from the planes, which have now only the external resistance to flight to overcome; and if we here assume that the velocity of translation is not less than the velocity of rotation, and may be greater, it clearly may be assumed likewise that the air abutment is no longer displaced bodily.

Then further, as the velocity of fall, or stroke, to sustain a given load, is assumed, and has been experimentally determined, to decrease as the velocity of translation increases; moreover, as the air abutment is not bodily displaced when the velocity of translation is not less than the velocity of rotation, it seems reasonable to assume that the motive power has here only to impart gliding motion to the planes upon their bed of compression, the elastic resistance of the bed of compression forming the sustaining resistance to the weight of body.

The rotating planes have to descend the height of what we may term the stroke, but they descend with only the resistance offered to progression in the line of their path.

As the sustaining air has to be compressed however, the extent of the compression would be equivalent to fall, unless the plane be inclined from the path *ed* Fig. 1.

The weight of body then pressing downward without energy, forms a balance to the weight of resistance pressing upwards on the plane face; and as by reason of the motion of translation, there is no bodily displacement of the air, the action of the rotating planes in easy flight upon their path of support is similar to that of birds' wings in soaring.

Large birds starting from a perch, or renewing forward impulse when they have been hovering, are seen to acquire sustaining velocity very quickly, by letting themselves with outstretched wings glide downward on an easy gradient; evidently showing that the acceleration of the forward impulse on the easy descent, balances by the increasing pressure upon the wings, the acceleration earthward due to gravity; the earthward energy of the weight of body being at zero in the start from perch or point of hovering.

The air pressure against which the outstretched wings of a bird have to be sustained, hinged at one end only, gives the measure of the tension due to pressure and leverage on the muscular power of the living wing, and the muscular power has to sustain this tension to the end of the stroke.

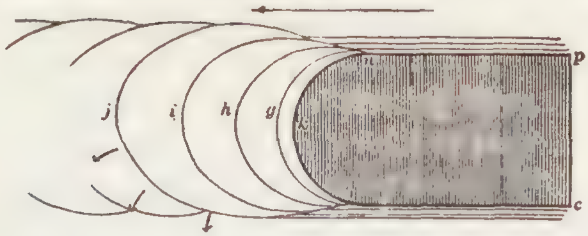
In the case of rotating planes spindled at both ends, the strain of the pressure appears at the spindles; and if the motive power be applied at the centre of rotation, we have the pressure acting with the leverage of the distance from the centre of rotation to the spindles; but in the cases of both bird and artificial plane, we have the angle of inclination in both wing and plane directing the burdened plane in the way of least resistance; and if the expansive reaction of the compressed air, before spoken of, be at work on the flexible rear edge of the plane, the motive or engine power required once a sustaining rate of flight is reached, may be slight.

As it has not yet been determined at what rate a wave of compression is propagated in air, precise value cannot at present be given to the expansive energy of the wave escaping to react from *a* to *b*, nor to the sustaining force on the faee *ca*, in relation to the force resisting the motion of flight on that face. We think it highly probablē, however, that the higher the velocity of the inclined surface that imposes the pressure, the less the distance the swell of the wave out from the surface in the time of passing, consequently the more dense will be the body of the wave of compression.

The motion *cd*, Fig. 1, would require to be motion of compression only, otherwise the expansive reation on *ab* would lack air support beneath; and that the motion *cd* is of compression only seems evident from the facility with which birds soar on an ascending path. Were bodily displacement of the sustaining air here to occur, the soaring wing would have to follow the air, to maintain the resistance, as the motion of bodily displacement implies that as regards the volume of air undergoing displacement, the force of inertia due to the rate of that motion has been overcome, and is no longer of avail for support to the weight possessed of that same motion.

It seems difficult in the case of an artificial wing to devise a substitute for the light and flexible feather tips that form the rear edge of a living wing; and it seems almost vain to think of artificial means of adjustment of the angle of inclination such as the sensitive living wing possesses; thus, it would be vain to think of continued flight for two dead wings of a large soaring bird, loaded with weight equal to that of the bird they had belonged to, and impelled artificially, as the living bird would be required to keep the angles of inclination in adjustment with the requirements of the weight that has to be sustained.

Fig. 2.



In Fig. 2, let *bcnp* be the end view of a narrow plane with its front edge rounded. The expanding fluid curves *g, h, i,* and *j.* represent the varying pressure of the air in front. The air in contact with the edge at *k,* is bearing the full weight of the imposed pressure due to the velocity; and as the advancing edge with its pressure is cushioned on the inert but elastic air in front, the force of inertia of the air that is continuously to renew the cushion as the plane advances, is developed gradually by reason of the elasticity, in such manner that, beyond a certain point in advance of *k* (the distance of which from *k* is dependent upon the V^2 pressure) the air is found at rest, the force of the pressure having been absorbed by the inertia of the weight of elastic air between this point and *k.*

Owing to the elasticity and compressibility of air, the effort of displacement has not to reach so far out as in the case of motion through water; moreover, in high velocities where little time is allowed for the diffusion of the pressure outwards, the wave of pressure, though more intense, will reach out a less distance than in low velocities; but the lateral expansion of the wave will depend greatly upon the length *bc* of the plane, because, the greater the length *bc,* the longer will be the time allowed for the expansive force to act outward for relief on the surrounding air before complete relief be got when closing in behind.

The compressive force is at its maximum at *k,* but is at zero at *b* because the surface there is rounded into line with

the direction of flight. The expanding fluid-curves *g*, *h*, *i*, and *j*, however, will have projected the pressure below the line of plane surface *bc*.

When the velocity is constant, the fluid-wave keeps uniformly in advance of *k*, but the curves which we have employed to represent the varying intensity of the pressure outward, for example the curve *j*, we may regard as uniformly flattening out upon the lines *de* and *lm*; and, in the act of flattening out, the air is put in motion in the direction indicated by the arrows; but this is a motion of compression mainly, and the faces *bc* and *np* bear the reaction in such manner that any slight projection on either of these faces would have to pass through a denser medium than opposes in the case of a thin plane.

The greater the velocity of flight, the more sudden will be the compression of the air in contact with the advancing edge, and the more compact will be the stratum of displaced air in contact with the faces *bc* and *np*; and, in the case of two or more planes slightly inclined and moving close together on parallel paths, the velocity of flight will determine what thickness of unmoved air will occur between them to form a bed of support to the weight of plane.

II.

Let the weight to be sustained by 1 sq. ft. of plane be 3·5lbs. The inertia force of 2·696lbs. of air pressure impelled at the rate of 41ft. per second is equal to that of 3·5lbs. at 32·18ft.; because as the plane is moving $\frac{41}{32{\cdot}18} = 1{\cdot}27$ times as fast in 1 second as the uniform acceleration of gravity, we have $3{\cdot}5 \quad 1{\cdot}27 = 2{\cdot}696$lbs. pressure at the 41ft. rate, doing as much constant work in the resistance of its inertia as 3·5lbs. at the 32·18ft. rate.

We will assume this to apply to a stationary stroke, on a

surface of air equal to the single area of the plane in motion; and to take the place of this force in a stationary stroke, we propose to put the inertia force of say 0·2319lb. of air pressure impelled at the rate of 11·85ft. per second, equal therefore to 0·0854lb. and possessed of motion of translation equal to the velocity of the stationary stroke, viz., 41ft. per second.

In the natural gravitation of the weight, neglecting air resistance, to acquire 11·85ft. velocity, it would fall 2·18ft. in 0·368 sec.; whereas it would require 1·274 seconds to develop 41ft. velocity, with a fall of 26·1ft.

The force of inertia can act only in the direction of the motion that develops it; and weight when translated consumes or absorbs applied force equal to that developed by an equal velocity in natural gravitation; moreover, the resistance of inertia is simply equal to the force expended on the given weight in imparting the given motion to it; so that, when, in a given time, that motion becomes uniform, the expended force is expressed in the motion.

The inertia resistance due to the given uniform pressure of a single area of air surface in the stationary stroke is overcome the moment the given uniform velocity of stroke is reached, for then the previously inert air has received the compression due to the velocity, and is in motion at the given rate; the sensible force upon the plane, however, is the static uniform pressure due to the density of the compressed air that forms the cushion of compression; and, as in displacement of air occurring in the cushion, the entering air would have transferred to it the inertia force of the air that is displaced, in the act of deflexion for displacement, it must necessarily follow that the force of inertia developed in the compression of the cushion determines the value of the support the weight receives in the stationary stroke; and any prolongation of the stationary stroke simply prolongs the duration of the support by maintaining the compression.

The work performed in the formation of the cushion requires the plane performing it to possess motion of compression at the given rate. The cushion when formed could throw out this work again upon external resistance, say back upon the plane, only by being allowed to expand to its natural density; but this motion of expansion is prevented by the plane maintaining the velocity to which the compression is due, so that the expansive energy pent up in the cushion formed is just equal to, so as to be balanced by, the impulsive energy of the sensible pressure of the plane in motion at the given rate.

The work performed by the plane in the act of compression is internal with respect to the cushion of resistance; and the motion of compression is at an end when the given weight of resistance in the cushion is reached, the cushion being then impelled bodily forward by the plane.

When the plane with its vertical stroke is translated horizontally, it strikes the air come upon at once with the given velocity of stroke, and in the time of the compression expends upon the air the energy that, in free gravity, in relation to the falling tendency of weight, would be developed in the time of the fall to which the velocity is due; and expends this energy in such manner that the plane would come to rest for the instant before reaction began, were this single effort of compression all that it possessed. The velocity of the plane is assumed to be maintained by the motive power at uniformly continuous rate, and we may assume, therefore, that the motion of compression takes part in the uniform velocity of the plane at the given rate; but as the force which the plane expends upon the cushion in the time of the compression is due to a velocity the value of which, as respects the weight supported, is represented by the time required in free gravitation to develop it, we have for the formation of the cushion

the time of the compression; and for the motion that imparts compression, the time of its development in the weight in a free fall; and as the cushion is on the surface, so to speak, of the body of air resistance, and receives in the time of the motion of compression the energy that takes the longer time required by weight freely gravitating to develop, it seems evident that, by motion of translation, to shift the plane on to fresh and unmoved air in the time of the compression, would give on the path of translation inertia support to the plane in a given time, say one second, equal to the number of times the time of compression was contained in one second.

The effort in compression determines the inertia value of the pressure on the plane at any point or moment of time in the path of the uniform motion; but, in the stationary stroke, we can estimate the value of the continued effort in the plane in motion only by the work done in a given time and fall.

The force of inertia developed in the cushion for a stationary stroke concerns the force of inertia that the supported weight of body would develop in the time for which the velocity is rated; so that, for one second, as the force of inertia due to 3·5lbs. weight at 32·18ft. velocity would be 3·5lbs., we have the force of inertia due to 2·696lbs. pressure at 41ft. velocity just balancing that of the weight.

To support the translated weight we have to give to the planes in the given time 1·0, the force that the weight itself would develop in that time by a free fall; but so that each unit of length in the area of the air path travelled by the plane in one second of time shall contribute resistance of inertia equal to the force which the weight would develop in the time occupied in travelling that unit of length if falling freely; in which case the weight of body as represented by its developed unit inertia would act in the manner of a load distributed over the path of resistance.

In the following terms we seek to show the relative equality of the different pressures at the different rates of translation, the effect of resistance to flight being here neglected :—

Actual energy in the given falling weight; or the work performed by gravity, and accumulated in the weight when the falling motion reaches the given rates of velocity in the respective times.

	Time ratio.	ft. vel.	ft. fall.	lbs.	Units' work.
$\frac{1}{2}$	0·637 sec.	– 20·5	= 6·56	× 3·5	= 22·96
	1·27 ,,	– 41·0	= 26·1	× 3·5	= 91·35
			1 to 4		1 to 4
$\frac{1}{3{\cdot}46}$	0·368 ,,	– 11·85	= 2·18	× 3·5	= 7·63
	1·27 ,,	– 41·0	= 26·1	× 3·5	= 91·35
			1 to 12		1 to 12
	1·0 ,,	– 32·18	= 16·0	× 3·5	= 56·31

and 56·31 : 91·35 : : 16·0 : 26·1.

Air pressure on the sustaining planes; the energy appearing in the resistance to the planes in motion, and not to the weight to be sustained.

Vel. ratio.	ft. vel.	Uniform pressure.	Uniform inertia.	Inertia ratio.
$\frac{1}{2}$	20·5	= 0·6797lb.	= 0·433lb.	= 2^3
	41·0	= 2·696	= 3·5	
		1 to 4	1 to 8	
		1^2 to 2^2	1^3 to 2^3	
$\frac{1}{3{\cdot}46}$	11·85	= 0·23lb.	= 0·0854lb.	= $3{\cdot}46^3$
	41·0	= 2·696	= 3·5	
		1 to 12	1 to 41	
		1^2 to $3{\cdot}46^2$	1^3 to $3{\cdot}46^3$	

The velocity and consequently the force of the plane are assumed to be constant during the stroke; and as a cushion of resistance equal in extent to the area of the plane must be formed during translation for every shift on to new air, we have the whole work done by the plane in one second, equal to its constant or uniform force, by the whole extended area of resistance that sustains compression in that time; or equal to the sum of its constant or uniform force per unit of area, on as many units of area of air resistance, successively undergoing compression, as the air path travelled in the given time contains; so that, employing the above inertia ratios 8 and 41, with their respective fall motions 6·56ft. and 2·18ft. to represent the fall motion to which the compression of each single plane area of cushion is due

$$\frac{22{\cdot}96}{8 \times 6{\cdot}56} = 0{\cdot}43\text{lb. for the } 20{\cdot}5\text{ft. velocity; and}$$

$$\frac{7{\cdot}63}{41 \times 2{\cdot}18} = 0{\cdot}08\text{lb. for the } 11{\cdot}85\text{ft. velocity.}$$

In natural gravitation free from air resistance, a falling body would require only 0·637 sec. to develop 20·5ft. velocity.

As we assume, however, that the body does not fall, and therefore does not acquire energy, but that the inertia force it would develop if allowed to fall appears in the air that resists the oscillating or rotating planes to which the falling motion has been transferred, we take the translated stroke in its relation to the stationary sustaining stroke, the sustaining force in which we found to be equal to the gravity of the weight supported.

If, however, the translated stroke, rated for 1 second, be taken in relation to the natural fall of the weight, we take for the 20·5ft. velocity one-eighth of 1 second for the time of action

of the weight on a single cushion of air; and the mean force developed in 3·5lbs. weight falling free from air resistance in one-eighth second would be 0·43lb.

The velocity of the fall here starting from zero is uniformly accelerated; whereas, we assume the velocity of the sustaining plane to be constant, so that we may take the mean energy of the weight, which gives us a fall of about 0·124ft. with energy about 0·43lb.

Regarding the 11·85ft. velocity, the work the 3·5lbs. weight would perform in an actual free fall of 2·18ft. would be 7·63; and, supposing the velocity, transferred to the plane, were by reason of resistance to become uniform at this point, and further, that the plane with its 0·085lb. inertia force would here begin successively to strike the particles of the air of resistance on the path of translation, so as to develop in them successively the constant inertia force of 0·085lb., the actual energy it would thus develop in the air would be equal to

$$2{\cdot}18 \times 0{\cdot}085 = 0{\cdot}1853; \text{ and } 7{\cdot}63 \div 0{\cdot}1853 = 41 \text{ times.}$$

If, however, we take the simple inertia force of the weight at the given velocity per second, and not the work done or accumulated, we then have for the 3·5lbs. weight 1·29lbs. inertia force; and $0{\cdot}085 : 1{\cdot}29 :: \dfrac{1{\cdot}00 \text{ sec.}}{41} : 368$ sec.; that is, the 0·0244 second time allowed per lineal foot in translation of the plane 1ft. square is in the same ratio to 0·368 sec. as 0·085lb. inertia force of plane is to 1·29lbs. inertia force of weight: so that $\dfrac{0{\cdot}368}{0{\cdot}0244} = 15{\cdot}08$; and $15{\cdot}08 \times \overset{\text{lb.}}{0{\cdot}085} = \overset{\text{lb.}}{1{\cdot}29}$, in the space of time the weight, if falling freely, would require to develop the given velocity of the plane; and this is at the rate of $\dfrac{1{\cdot}00}{0{\cdot}368} = 2{\cdot}72 \times 1{\cdot}29 = \overset{\text{lbs.}}{3{\cdot}5}$ for 1 whole second.

The plane with its vertical pressure of sustaining resistance has to be deflected horizontally along the path of translation; then, as $2{\cdot}696 \times 41 = 110{\cdot}5$ units of work done upon the air in the stationary stroke; so $0{\cdot}6797 \times 20{\cdot}5 \times 8$; and $0.23 \times 11{\cdot}85 \times 41$, respectively, give approximately the same work in 1 second, neglecting differences due to the co-efficient of resistance independent of velocity; so that, as the inertia force developed vertically is represented by that of the vertical pressure on the sustaining plane, we have only to suppose the plane with this pressure to be carried the two distances, vertical and horizontal, to get the external work done upon the air in 1 second. In the resultant we have the two motions joined, but have the energy of the vertical stroke acting with full force along the whole length of the space of translation.

The velocities are here assumed to be developed; and to bring the work done upon the air into just relation to the work in gravity performed by the 3·5lbs. weight allowed to fall 1 second, and which the former has to balance, we employ the velocity that would naturally be developed in 1 second, assumed then to become uniform, and $3{\cdot}5 \times 32{\cdot}18 = 112{\cdot}63$ units of work.

The air pressures due to velocity of plane are according to Morin's Rule.

In the case of a bird, the energy of the weight acquired in a vertical fall, would be used to give velocity to the translating deflexion, free from wing stroke; but this would be only where there was room for the necessary convexity earthward of the curve the path would lie in.

The vertical pressure on the wing would decrease with the lessening vertical momentum of the weight. In a horizontal line of flight, the air resistance developed on the wings takes the place of the force of gravity that would be developed in the weight if left free to fall; so that in travelling forward, the

wings distribute the equivalent to this force of gravity on an extended bed of air; and the quicker the flight, the more extended is the bed of air resistance, in a given time, and therefore the less the sustaining resistance required per unit area of the extended bed; consequently less pressure is needed upon the unit area of the wing. The weight can cease to exert pressure on the wing only at such moments as correspond to the summit of the rise of a body shot up into the air. The flight of some birds shows a quick succession of such summits, with low elevation from the points between on the undulating path of flight.

In the case of a bird swooping downward, and then in the path of a curve convex to the earth, employing the momentum it has acquired in the fall to carry it upward again, the wings will be inclined tangentially to the curve, so that if the curve be regular, perpendiculars to their faces would all meet at one point inside the curve.

III.

From tabular records of experiments made by M. Didion in conjunction with Morin, and recorded in Bennett's "Morin," we extract a few of the quantities relating to the descent in air of a plate 1·196 square yards, area, = 10·7641 square feet; the quantities relate to the latter part of the fall. Columns *S*, *T*, and *V*, are derived directly from the table given, and the quantities in the other columns are got by process of simple subtraction.

Let S be the whole space fallen from the starting point to the point of observation; T the whole time of the fall to that point; V the velocity acquired on reaching each successive point; $\frac{v}{t}$ the ratio of acceleration the successive intervals between the observed points; v, t, and s, respectively represent the velocity, time, and space, of these successive intervals.

No.	S	T	V	s	t	v	$\frac{v}{t}$
1	11·975ft.	1·187	18·21				
2	14·429 ,,	1·346	19· 5	2·454	·159	1·29	8·113
3	17·992 ,,	1·493	20·73	3·563	·147	1·23	8·367
4	20.991 ,,	1·636	21·75	3·000	·143	1·02	7·133
5	23·989 ,,	1·771	22· 5	2·988	·135	0·75	5· 55
6	26·988 ,,	1·910	22· 8	2·999	·139	0·30	2·158
7	29·439 ,,	2·304	22·86	2·451	·124	0·06	0·484
				17·455			

Thibault, experimenting with two planes, each of 0·12323 square yards area, and at the end of arms each measuring 8·97ft. mounted on a horizontal axle, and motion given in all cases by the descent of a weight of 8·82lbs. got results represented by the proportionate quantities in the following table.

In the table as given by Thibault, the quantities are expressed in yards and lbs.; but, to exhibit how nearly the sine of the angle of inclination gives the measure relatively of the resistance, up to 50°, we find it convenient here to express the quantities in the proportion they bear to 1·0 for 90° which is the angle when the plane is perpendicular to the direction of the motion, and therefore the angle of maximum resistance.

As the motive power employed to move the axle with its curves and planes, was in all cases produced by the descent of

a weight of 8·82lbs. it followed that the less the resistance on the planes, the greater the velocity of revolution; hence, in the table, the decrease of the resistance is in inverse proportion to the increase of the velocity; but, as the decrease of the resistance is owing to the lowering of the angle of inclination from the maximum 90°, the area of the plane being reduced thereby from its full value 1·0 for 90°, to a value given by the sine; so that, at every change of angle, and consequent change of velocity, as the value of the area is altered, we can most readily exhibit the differences in resistance by the differences in velocity simply.

The column V gives the simple velocity in yards per second; and the column V^2 the velocity in yards squared.

In the column aV^2 to exhibit the decreasing resistance in relation to V^2 we make V^2 for 90° = 1·0; and divide this 1·0 by the successively greater quantities V^2 of the lower angles, to get their inverse ratio, the ratio thus expressed decreasing approximately with the decrease of the resistance per yard of velocity.

The column A gives the sines of the angles, and consequently what is termed the perpendicular projection of the inclined plane, or the value of the area of the plane when inclined, as a displacing area.

In column B the ratio of the resistance to the square of the velocity for 90°, thus—

$\dfrac{C \text{ for } 90^\circ}{V^2 \text{ for } 90^\circ} = \dfrac{0{\cdot}1563\text{lb.}}{7{\cdot}5735\text{ yds.}} = 0{\cdot}02063\text{lb.}$ per square yard of velocity is represented by 1·0; and the lower resistances for the greater velocities due to the angles below 90° are divided by this 1·0, to get the proportion they bear to the 90° maximum rate.

In the column C, the resistance, = 0·1563lb. proportioned to V^2 for angle of 90 is made 1·0; the lessened quantities

represent the proportion that the lessened resistances bear to this 1·0.

In the column D the actual resistance $= 0{\cdot}166$lb. for the given area and velocity at 90° is made 1·0. If the ratio of resistance to displacement area represented by the sine were uniform, the quantities for the successive angles would be 1·0 uniformly, increase of V^2 compensating for loss of projected area.

Column E gives the rate of the resistance per square yard of displacement area, represented by the sine of column A, and per yard of velocity; thus, as the actual area of the plane was 0·12323 square yard, we have

$$\frac{1{\cdot}000 \text{ square yard}}{0{\cdot}12323 \quad ,, \quad ,,} = 8{\cdot}114 \text{ times, and}$$

$B \times 8{\cdot}114 = 0{\cdot}02063 \times 8{\cdot}114 = 0{\cdot}16737$lb. for E at 90°

The fluctuations occurring between 90° and 50° are evidently due to mechanical irregularities in the working of the apparatus.

Column E shows readily where what, for shortness, may be termed slip, begins sensibly to reduce the ratio of the resistance as the decreasing angle approaches 0°; the column of sines the while representing the air displacement value of the inclined plane surface.

It will be seen that in angles below 50°, the rate of resistance declines rapidly.

The quantities that head the table, with the exception of the 1·0 of column aV^2 are in lbs. and yards as given by Thibault, and are for the maximum resistance for 90°.

The observations were taken when the resistance of the air on the inclined planes, so balanced the motive power of the descending weight, as to make the velocity uniform; and column T gives the time in seconds taken to make 20 uniform revolutions of the axle with the planes at the given angle.

T		V	V^2	aV^2	A	B	C	D	E
Time of 20 revolutions.	Angle	Vel. yds.	Vel. yds.	Ratios	Sines.	Ratios Resistance to V^2	Resistance proportional to V^2	Total actual resistance on projected area.	Resistance per sq. yd projected *i.e.*, as per sine and per yard velocity.
secs.		yds.	yds. sq.		sq. yds.	lbs.	lbs.	lbs.	lbs.
68·40	90°	2·752	7·5735	1·000	0·12323	·02063	0·1563	0·1660	0·16737
					1·00	1·00	1·00	1·00	1·00
67·90	80°	2·772	7·6839	·9856	·9848	·9844	·9987	·9988	0·9995
65·56	70°	2·828	7·9973	·947	·9396	·9442	·9968	·997	1·005
62·47	60°	3·014	9·8041	·8337	·8660	·829	·9942	·9945	0·957
60·25	50°	3·124	9·7593	·7760	·7660	·7697	·9923	·9927	1·005
52·83	40°	3·563	12·6949	·5965	·6427	·587	·984	·985	0·913
43·00	30°	4·378	19·1668	·3951	·5000	·3805	·9629	·965	0·761
30·50	20°	6·173	38·1059	·199	·3420	·1847	·929	·9331	0·540
24·50	15°	7·683	59·0284	·1283	·2588	·1076	·8407	·850	0·416
19·00	10°	9·91	98·2081	·0771	·1736	·0552	·7184	·735	0·319

Employing the lb. and yard quantities given for 90° at the head of the table, we will here show their mutual relation, and will distinguish the several quantities by the letters that head their respective columns: thus—

$$A V^2 E = C.$$

$$\underset{A}{0{\cdot}12323} \times \underset{V^2}{7{\cdot}5735} \times \underset{E}{0{\cdot}16737} = 0{\cdot}1563 = C.$$

$$\frac{C}{V^2} = B$$

$$\frac{0{\cdot}1563}{7{\cdot}5735} = 0{\cdot}02063 = B$$

$$\frac{1{\cdot}0 \text{ sq. yd.}}{A} \times B = E$$

$$\frac{1{\cdot}0}{0{\cdot}12323} \times 0{\cdot}02063 = 0{\cdot}16737 = E$$

IV.

Any solid body while in flight, tends to leave a more or less partial vacuum behind, according to the speed; and, as the removal of one unit of pressure on the rear face, leaves one unit of pressure on the front face unbalanced, and therefore free to act backward, the impelling force has to expend part of itself in overcoming the backward tendency; but even in the utmost velocity attained by fleet birds, the unbalanced pressure is inappreciable, and may be neglected in the case of mechanical flight, seeing that the rule given for the determination of resistance to planes in motion is derived from the results of experiments in which this backward tendency was in action. We may however explain the reason of the pressure on the rear face being less than the normal atmospheric pressure.

Air rushes into a vacuum with the velocity due to a body falling freely in space, from the height of a column of air that would give the nearly 15lbs. static pressure per square inch at the sea level, assuming the density of the air in all that height to be uniform with the density at the sea level: thus, the weight of one cubic foot of air 62° Fah. is 0·0761 lb., and the height of column of air required is about 27835 feet, $= H$; then $\sqrt{H \times 2\,g} =$ velocity at end of fall, which gives 1338 feet velocity of air entering a vacuum, when the barometer is at 30.

We will suppose a body, the front face of which measures 1 square foot in area, to be in motion, and will assume for it the velocity of 44ft. per second, equal to 30 miles an hour, with the pressure of 3·1049lbs. per square foot of area.

The static pressure of the atmosphere being nearly 15lbs. per square inch, is equal to 2169lbs. pressure per square foot, and $\frac{2160\text{lbs.}}{3{\cdot}1049\text{lbs.}} = 696$ times the atmospheric pressure exerted on a vacuum exceeds the pressure of resistance to the motion of the body at 30 miles an hour, or 44ft. per second; and $\frac{1338}{44} = 30{\cdot}4$ times, so that, as the air is capable of rushing with 30·4 times the velocity of the body, it follows that, if the body moves 1 unit of distance, the air will close in behind expansively, and have $30{\cdot}4 - 1{\cdot}0 = 29{\cdot}4$ times velocity still in reserve undeveloped; and $1338 - 44 = 1294$ft. per second reserve velocity; and $\frac{1294}{8{\cdot}02} = 161{\cdot}34 =$ the square root of the height of column representing the height of fall for the reserve velocity, $161{\cdot}34^2 = 26030{\cdot}59$ feet fall H^1, that would generate the reserve velocity.

Now it is clear that as the reserve velocity here is undeveloped, the force of the developed velocity can be no more than is due to 44ft. per second; and, as the body is assumed to be receding at that constant rate, and further, as the force which is following behind to fill the vacuum is satisfied with the simple filling and the restoration of the atmospheric pressure behind, to balance that in front, and can add no impetus to the receding body, else would the force in the receding body be augmented beyond the power that produced the vacuum, any increase of velocity occurring in the receding body can only simply develop more of the reserve velocity in the air closing in behind, until, when the velocity of the body and of the air behind are equal, the air has done its utmost; and, if the body increases its speed still more, as in the case of a gun ball, the air cannot keep pace with it, a vacuum is the consequence, and the body has now to sustain on its front face the unbalanced

pressure of the air at the rate of nearly 15lbs. per square inch of perpendicular surface, in addition to the resistance its velocity excites by the compression of the air on its front face.

As shown in the reduced height H^1 of the air column to which what we have termed the reserve velocity is due, when the air has expended 44ft. of its 1338ft. full vacuum rate per second, the pressure per square foot appearing in the elastic current, that in the manner of expansion of the surrounding air is closing in behind at this lesser rate, can be no more than that due to H^1 equal 26030·59ft.; the difference will have expended itself in motion; and, when the full rate of 1338ft. has been reached for the absolute vacuum, the original whole pressure of 2160lbs. per square foot at the starting ing point, will likewise have expended itself in motion, else would it have power to keep pace with the receding body at a velocity beyond this.

The height of fall that would give the determined velocity is the height of the column of air whose weight gives what is termed the atmospheric pressure; but we have this atmospheric pressure appearing in the vacuum and acting with expansive elasticity in the manner of the elasticity of a depressed spring when the depressing resistance yields to it.

The air enters the vacuum equally from all directions with the parts nearest to the vacuum made thinner by the immediate expansion, than those farther away in the body of the surrounding air, but if the body be moving horizontally at a velocity of say 1400ft. per second, it is clear that, though the air can readily close in behind on transverse lines radial to the axis of flight, the rear face of the body in motion, receding $1400 - 1338 = 62$ft. per second faster than the air can follow along the axis of flight, will be free from pressure.

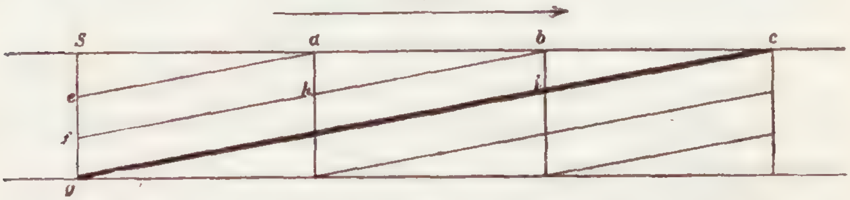
It is evident, however, that a vacuum can never be formed except at velocities far beyond what can ever be attained by

wings; and as these high velocities belong to gunnery rather than to aërial locomotion, moreover, as the co-efficient of resistance is found sensibly increasing with the velocity in high velocities, moderate velocities with the co-efficients relating thereto need alone be here considered.

V.

In Fig. 3, let *cg* be a plane inclined and in motion in the direction indicated; and let the points *a, b,* and *c* be points successively reached by the leading edge in equal intervals of time.

Fig. 3.



We will suppose the plane to have started from *S* at a given uniform quick motion, and on reaching *a* to have displaced a volume of air from *S* to *e*, in volume equal 1, and with 1 unit of force. On reaching *b*, a similar volume of air equal 1 will have been displaced from *a* to *h*, while the first volume will have been displaced further from *e* to *f*; and as the motion is uniform, and *Se, ah, ef,* &c., are all similar spaces of displacement, it is evident that the displacement force in *ef*, will at *f* be equal to 2 units $= Se + ef$; and at *g* will be equal to 3 units; while the force expended on the displacement of the volume from *b* to *j*, will at *j* be only 1 unit, same as at *e* and at *h*.

At *g* however, the units of force are due to 3 corresponding units of time; at *f* to 2 units, and at *e*, and *h*, and *j* to 1 unit of time only; but as the spaces *Se*, *ef*, *fg*, &c., are equal, and correspond to the similarly equal spaces of area, *ae*, *bh*, *cj*, &c., we have 1 unit of time and of force to each unit space of area.

The unit volume of air on acquiring the given uniform velocity at *e*, does not within itself develop more than the given unit of force; but the resistance to displacement of the air beneath it, and against which it is pressed, develops the second unit of force due at *f* in a second volume.

It is apparent that the stratum of free air here displaced by compression, must act differently to a similar bulk confined; the angle of inclination of the plane is assumed to be low, and the velocity of flight to be rapid, to give short time for the compressed air, by the effort of expansion to overcome the inertia of the air beneath the path of flight.

We assume that there is no lateral relief, and that the only direction of displacement is perpendicular to the face of the plane.

Allowing that there be 3 volumes of air at *g*, each possessed of 1 unit of force in elastic compression, the expansive reaction when the 3 volumes are liberated to the rear of *g*, will carry the wave of expansion farther than the wave from a single volume set free to the rear of *e*; and upon this expansive reaction would depend the forward impulse on the flexible plane of Fig, 1.

We have assumed 3·5lbs per square foot of wing plane for the weight to be sustained. This proportion of weight to area is in excess of that observed in birds, and would necessitate greater velocity of flight, which would be more readily attained if the greater burden were relatively in smaller bulk than the lesser burden with its lower velocity.

The bulk of a man weighing 154lbs. is, roughly, about 4 cubic feet; and a bulk of air of this weight is about 1909 cubic feet at 32° Fah.

A cube measuring about 1·59ft. length, depth, and breadth, would represent the man; and a cube measuring 12·4ft. on the side would represent the air; and 1909 ÷ 4·0 = 477 times the volume of air exceeds the volume of the man.

Were the man able to expand his form so as to occupy the 1909 cubic feet equivalent volume of air, he might float with the same motion as the air surrounding him; but, if dissatisfied with that passive motion, he sought to transport himself afloat from place to place independently, he would find that the air sought to be displaced in this motion of his own, had as much gravity as he himself possessed.

If, with this 1909 cubic feet expanded volume and weight still equal to the air equivalent, he shaped himself say into winged form, the winged form would present less frontage for the air to act upon in resisting forward motion; but, the outspread area of the wing-planes would now present so much more surface for air currents to take hold of, that, if he purposed going to any particular place, he would have to wait till the wind went that way likewise.

If, with his weight still equal to the air equivalent, but his expanded form contracted into half the space of the air equivalent, that is, from 1909 to 954·5 cubic feet, his buoyancy would be gone; and as he now occupied only half of the equivalent air space, only half of his 154lbs. weight would be supported by the surrounding air; the other half, equal 77lbs., unsupported would take him direct to earth, with his descent retarded only by the resistance to displacement due to his contracted or half volume form.

If, however, while up in air, he could give motion to this contracted form, the momentum of the 77lbs. free weight

would enable him to cross a current of air with less side drift than if his form were expanded to the 1909 cubic feet equivalent volume, in which his full weight of 154lbs. would be wholly buoyant; because the 77lbs. free weight would here relatively be weight without form for the resistance of the air to act upon; and the air in resisting displacement, would have only the contracted or 954·5 cubic feet volume, representing the 77lbs. floated or buoyant weight to deal with; thus, we may suppose *A* in Fig. 4 to represent the 154lbs. wholly buoyant; and *B* to represent 77lbs. buoyant, with the 77lbs. free weight enclosed in it as *C*.

Fig. 4.

JAMES ARMOUR.

NOTES FROM FRANCE,

BY

T. J. BENNETT.

ENGLAND is not the only country that can boast of an Aëronautical Society, for France possesses one which is worthy o. the land where Aëronautics first saw the dawn. In 1863 all Europe was roused and interested in the solution of the problem of aërial navigation, by the energetic appeals of MM. Nadar and La Landelle. An Aëronautical Society was founded, and for a year or two flourished; but as nothing practical was forthcoming, it soon languished and died. The present Society was founded in 1868 by Dr. Hureau de Villeneuve, to whom, in a great measure, its present flourishing state is due. This energetic gentleman also started a monthly magazine, called *l'Aëronaute*, which has continued to appear regularly ever since April, 1868, and is the only instance in which a journal specially devoted to aëronautical science has been a continued success.

The Society meets twice a month to read and discuss papers on every subject connected with aëronautics. They also

possess an excellent library of aëronautical works, and a museum of aërial models, which is open daily, at the residence of Dr. de Villeneuve.

A summary of the transactions of the meetings is published in *l'Aëronaute,* along with the most important papers read.

Many of the members of the Society are passionately devoted to the solution of the problem, and have spent much time, ingenuity, and money in experiments, with considerable success. Most prominent amongst them is M. Penaud, who has succeeded in constructing models that will fly on three different principles, viz.—the vertical screw, an aëroplane with automatic rudder, and a mechanical bird with flapping wings. We purpose giving a description of these machines, with illustrations, thinking they will be of interest to the members of our Society.

We will begin with the hélicoptere, or vertical screw, as being the most simple and possessing the greatest antiquity. The first machine on this principle was constructed by MM. Launoy and Bienvenu, and presented to the French Academy in 1784. It consisted of two vertical screws superposed, turning in contrary directions. The motive power was a whalebone bow attached to one of the screws, two strings proceeding from its extremities to the vertical shaft of the other screw, round which they were wound. The reaction of the elastic bow produced the contrary revolutions of the screws. This little model rose to the ceiling, lifting a load equal to its own weight. Although this was the first working model of a hélicoptere that we know of, the principle had been proposed as far back as 1768, when Paucton in his treatise on the Théorie de la vis d'Archiméde describes a machine provided with two screws which he calls a ptérophores. One of the screws was for ascension, the other for propulsion. Sir George

Cayley also constructed a machine similar to that of MM. Launoy and Bienvenu in 1795, which he described in Nicholson's Journal for April, 1810. Deghen in 1816, Oittoris Sarti in 1823, and Dubochet in 1834, all proposed and constructed models for flying machines on the vertical screw principle. The idea then seems to have died out till, in 1863, MM. Ponton, d'Amécourt, de la Landelle, and Nadar drew the public attention to the application of the screw to aërial navigation, by exhibiting several models, driven by clock springs, which ascended to the height of from nine to twelve feet, with graduated weights attached to them. It is is only due to our fellow-countryman, Mr. Bright, who is, I believe, a member of this Society, to state that, in 1859, he took out a patent for a machine to be sustained by vertical screws, and constructed a model, which is to be seen at the Patent Museum, Kensington. Many others, including Mr. Bourne, the well-known engineer, have also constructed flying models on the same principle. Nearly 40 years ago Mr. Artingstall, of Manchester, constructed a machine driven by compressed air, but did not succeed in making it self-supporting. Encouraged by the success of his spring models, M. d'Amécourt had a small steam engine, with an aluminium boiler, constructed and provided with a pair of vertical screws, but it was not very successful, only lifting about a third of its own weight. This model was shown at the Exhibition held at the Crystal Palace in 1868.

All the spring models which we have described were so delicate that they were often broken on descending to the ground. Their flight only lasted a few seconds, and partook more of the character of an aërial somersault than true flight; for they had no sooner commenced to ascend than the spring had run down, and the screws stopped. These defects struck M. Penaud, who made many experiments to overcome them. Whalebone and steel springs only store up a very small power

compared to their weight, so he decided to use indiarubber, which is far more powerful; but if the rubber is used under tension it requires an immensely strong and heavy framework to stand the pressure: M. Penaud therefore determined to use its elasticity under tension, which greatly simplified the mechanism. It also possesses the remarkable quality of developing an almost uniform power without the intervention of compensating machinery. The immense advantage in employing rubber in the place of steel springs, is evident by comparing the following data :—

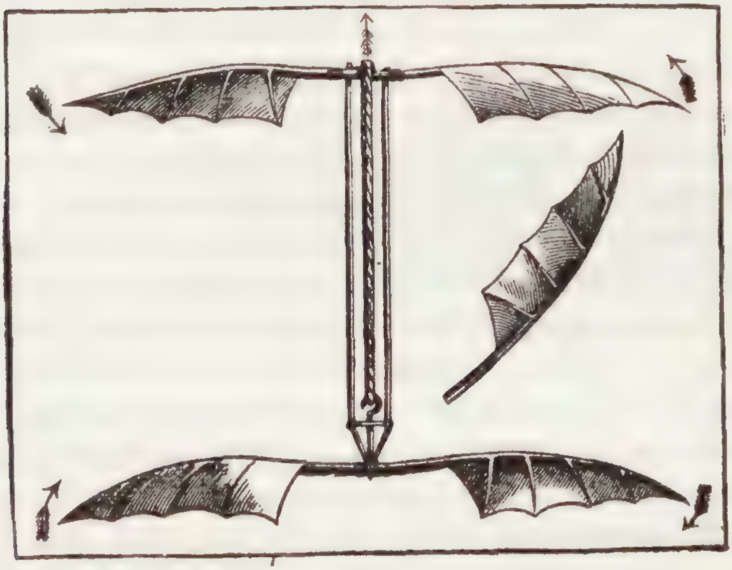
A steel spring weighing 1 kilogramme (2lbs. 3½ozs.) will only store up a power of 73 foot lbs., while the same weight of rubber stretched to six times its natural length will give out in contracting a power of 3660 foot lbs., that is to say fifty times as much. But in order to utilize the tension of rubber it would require a mechanism more or less complicated, which would absorb part of the power and be of considerable weight. By using the rubber under torsion the mechanism is extremely simple, the elastic being connected directly to the screws; but it has the disadvantage of furnishing only a power of 1300 foot lbs. per kilogramme. The power developed under torsion is thus greatly inferior to that given off under tension; but this inferiority is in great part compensated for in small models by the simplicity of the mechanism and the uniformity of the power given off.

M. Penaud first applied his new motive power to a helicoptére or vertical screw which is represented in Fig 1.

This model was constructed and shown to the French Society in 1870. It consists of two screws superposed, turning in contrary directions; their distance apart being maintained by two little strips of wood between which is placed the rubber. One end of the rubber is attached to the frame which carries the top screw, and thus turns it by reaction; the other end is

fastened to a hook on the extremity of the shaft, to which is attached the bottom screw, thus causing it to revolve by direct action in a contrary direction to the top one.

Fig. 1.



In order to fly the model, the frame is held by the left hand and the lower screw turned by the right one in a contrary direction to that which is requisite to support the machine. When the rubber is sufficiently twisted, it is only necessary to abandon the apparatus to itself. It will then (according to the proportion of the screw area to the weight) rise like an arrow to the height of 50ft.; glide obliquely in describing large circles, or else, after having mounted to the height of 8 or 9 yards, hover in the same place for 15 or 20 seconds, and even for 26 seconds.

M. Penaud has also experimented with a metal screw rotated by a string after the manner of the well-known toy,

the flying top. The late M. Babinet, the celebrated mathematician, stated some years ago in a lecture on Aërial Navigation, that he had seen one of these toys fly over the cathedral at Anvers; but M. Penaud has surpassed this feat by means of a screw well polished and silvered. The inclination of the blades was only from 3° to 5° from the horizontal. When started in a slight wind it will rise slowly for 4 or 5 yards and then go off in a horizontal direction with increasing velocity for 60 or 70 yards. It then ascends rapidly, often disappearing in the distance, when in a few seconds it will reappear approaching its starting point at a height of 60ft., and with the rapidity of an arrow dash over the experimenters' head to the distance of 100ft. in the opposite direction. The total time occupied in this erratic flight being about 20 seconds. This toy is rather a dangerous one to fly, but the experimenter is well repaid for the risk run by its marvellous flight, which demonstrates that a simple screw suffices for the support, the translation, and the equilibrium.

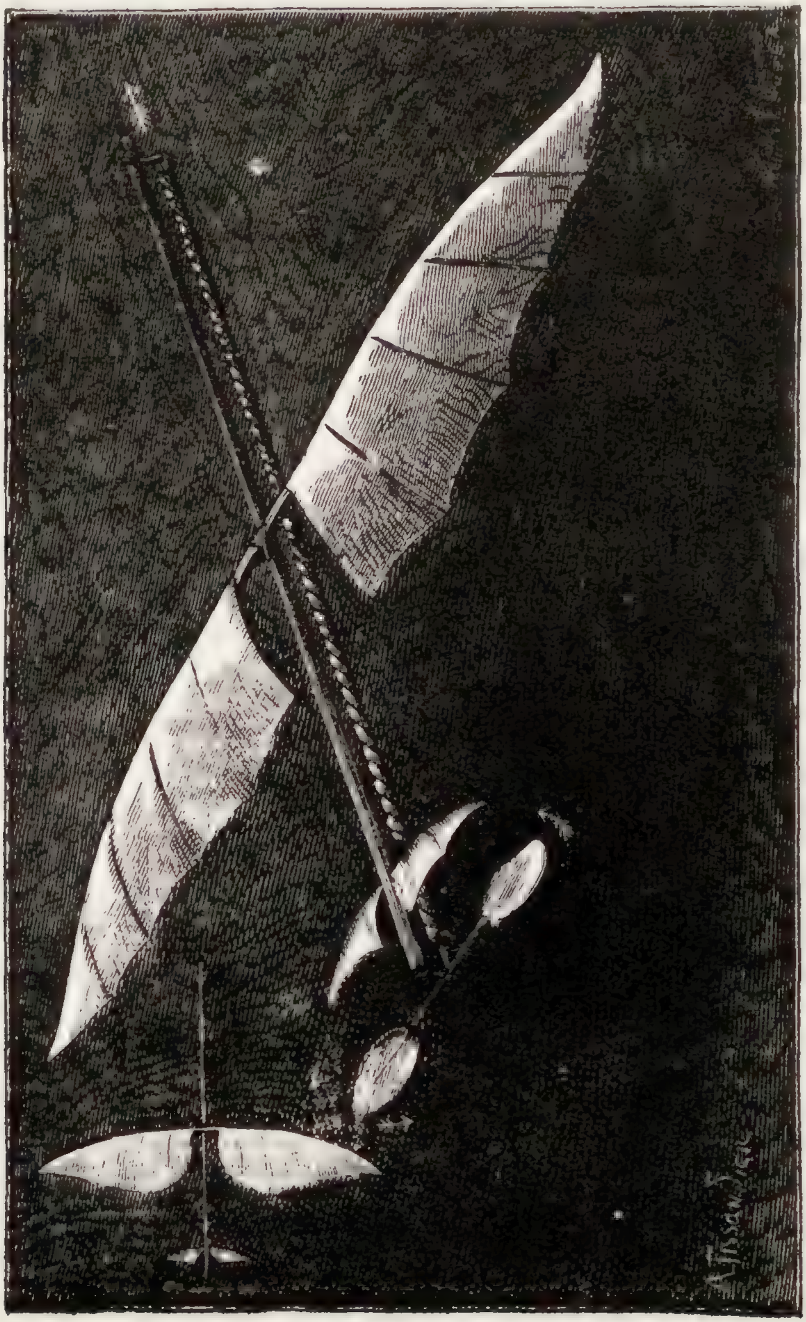
A few experiments have been made in France with large screws driven by manual power. In 1863 M. La Landelle constructed a screw 20ft. in diameter, with which he was able to support a weight of 32lb. when the machine and man were placed upon a weighing bridge. He afterwards tried it with the screw free to move in a vertical line on the shaft, after the manner of Mr. Wenham's screw, described in the First Report of this Society, when he found it would rise along the shaft with a weight of more than 100lb. attached to it. This weight evidently cannot have been supported by the air, for in the former experiment he was only able to lift 32lb. The phenomenon was caused no doubt by some peculiar action of the shaft on the screw. The above experiment would tend to throw some doubt on the reliability of the results given by Mr. Wenham's screw.

Two years ago M. Renoir, a member of the French Society, experimented with a screw 15ft. in diameter, with which, by the action of his feet, he was able to lift a weight of 26lbs. The screw was two-bladed with an increasing pitch, the angle of inclination being 3° at the front edge of the blade and increasing to 30° at the back edge. The two blades cover the entire area of the screw and have a deep rim suspended from them to prevent the air being driven from the circumference by centrifugal force. M. Renoir estimated the power he developed was about one-fifth of a horse power, but this was considered, by the members of the French Society present at the experiment, to be considerably below the real power exerted. As the screw was driven by the feet after the manner of a velocipede, the body being in a good position for exerting its maximum effort, the power delveloped was undoubtedly nearly 1 horse power. A man running up a pair of stairs is able for a few seconds to exert two-horse power, and mounting a ladder placed vertically, by the help of his hands, an ordinary man can do the work of 1¼-horse power. These facts have been determined by experiment.

As we have now exhausted the subject of the vertical screw we turn to the aëroplane. Sir George Cayley, in 1810, proposed a machine which consisted of a flat surface inclined up at a slight angle, and propelled horizontally by a screw propeller; but he did not go further than prove its practicability on paper. Henson, in 1842, patented a machine on the same principle, and his fellow-workman, Mr. Stringfellow, succeeded, in 1847, in making a small steam engine, provided with an aëroplane, fly. In France M. du Temple, in 1857, and M. Jullien, in 1858, constructed small models which were successful. M. Jullien's model weighed 36 grammes (1¼oz.), and its sustaining planes were 40 inches from tip to tip. It was propelled by two two-bladed screws, the motive power being

a piece of elastic under tension. The machine flew for five seconds, during which time it covered a distance of 40ft. But all the above models flew by accident, there being no special means provided for maintaining the equilibrium fore and aft. This problem M. Penaud has solved by means of his automatic rudder. Having proved the power of the vertical screw he thought of applying his rubber to a machine on the aëroplane principle, but was for some time baffled by the difficulty of maintaining the equilibrium. At last the idea occurred to him of placing a small horizontal rudder behind the sustaining planes, and inclined at a small angle to it. It succeeded perfectly. Its mode of action is as follows:—The centre of gravity of the machine is placed a little in front of the centre of pressure of the aëroplane, so that it tends to make the model descend an incline; but in so doing it lessens the angle of inclination of the aëroplane, and the speed is increased. At the same time the angle of the horizontal rudder is increased, and the pressure of the air on its upper surface causes it to descend; but as the machine tends to turn round its centre of gravity, the front part is raised and brought back to the horizontal position. If owing to the momentum gained during the descent the machine still tends upwards, the angle of the plane is increased and the speed decreased. The angle of the rudder from the horizontal being reduced, it no longer receives the pressure of air on its superior surface, the weight in front reasserts its power, and the machine descends. Thus, by the alternate action of the weight in front and the rudder behind the plane, the equilibrium is maintained. The machine during flight, owing to the above causes, describes a series of ascents and descents, after the manner of a sparrow. The lateral stability is easily obtained by slightly inclining the aêroplanes upward from their bases, or even by just turning up their tips. The machine is represented in Fig. 2, and after what we have

Fig. 2.



said about the hélicoptere its action is seen at a glance. It consists of a rod 20 or 30 inches long, which constitutes the main frame. To its front end is attached a small hook, and to the back one a bearing for the screw axle, which is also terminated by a hook. Between these hooks the rubber is stretched. The screw is two-bladed, to prevent injury to it on striking the ground, and is 8in. in diameter. In the illustration it is shown at the back part of the machine, but it has been placed in the front with equal success, only in the latter position it is subject to be damaged on striking an obstacle. Some models have been made with a pair of screws turning in contrary directions, to prevent the reaction of the elastic turning it over sideways; but this is easily prevented by fixing a small piece of lead to the outer extremity of the aëroplane. About the centre of the rod is placed the sustaining planes, which are made to slide along it to any position. The angle of either plane can also be altered at will. A little distance in front of the screw is placed the horizontal rudder, which is inclined upwards. The length of the plane is from 18in. to 2ft., by about 4in. in width. Its ends are slighly turned up, as are also those of the rudder, in order to maintain the lateral balance. The centre of gravity is a little in front of the centre of the aëroplane. A model constructed on the smallest of the above proportions weighs 16 grammes (1½oz.), of which the elastic represents abou one-third. In order to fly the machine it is necessary to wind up the elastic by turning the screw about 240 times. Upon abandoning it to itself, in a horizontal position, it will fall about two feet, but at the end of this descent, having acquired velocity, it rises, and flies at a height of eight or ten feet from the ground for about 130 feet. This distance is accomplished in 11 seconds. Some of the large models have even flown for 200 feet, supporting themselves for 13 seconds. During the whole of the flight the horizontal

rudder maintains the equilibrium in causing it to describe isochronal ascents and descents. When the rubber has nearly run down the apparatus descends gently to the ground, taking an inclined course, and preserving its upright position. The mean speed is 12 feet per second, which is fully equal to that of any insect provided with the same proportion of wing surface. If the model is started against a wind equal to its own velocity it will remain suspended in the air similar to the hovering of a bird. Great practice is required to start the machine with the wind on the beam, as it gets under the plane and tends to turn the model over sideways. When flown with the wind it must be thrown forward like a dart. Its velocity then of course is equal to the sum of its own and that of the wind. If, in experimenting, the model pitches forward on its nose, it is only necessary to slide the aëroplane further forward on the rod. If it still pitches turn up the horizontal rudder slightly. A little experience will soon determine the proper angle. Each plane consists of a long quill, to which the stems of smaller feathers are attached by means of pins pushed through the main quill and down the centre of the small one. The whole is covered with gold-beaters' skin. They should be inclined upwards at an angle of about 7°. M. Penaud presented his first aëroplane to the French Society in 1871, since which period he has often exhibited it to the public. From calculations and experiments with this model he thinks that one-horse power would support about 85lbs. He has also succeeded in constructing boomerangs in steel and wood, after the model of the Australian ones, which fly equally as well. Their peculiar flight is owing to their shape, which is that of a descending screw.

The following details of an aëroplane on a scale large enough to carry a man, now in course of construction at Brest, by M. Du Temple, may be of interest. It consists of a plane 40ft.

from tip to tip, and two rudders, one horizontal and the other vertical. The frame is made tubular and of steel, the whole being mounted on three light wheels. The motive power is a hot air engine; the two cylinders 18in. in diameter, being, constructed of thin steel, strengthened by rings of the same metal. The cylinder covers carry the piston guides, and are also provided with safety valves. The bottom of the cylinders are exposed to the fire, the fuel being petroleum. The machine is propelled by one six-bladed screw 13ft. in diameter. The total weight is 160lbs. The whole of the workmanship is very fine, no expense having been spared, and when finished will cost not less than £1200. This machine has been building for some years, but is now nearly finished, so that we may hope soon to hear of its "going off."

We will now deal with the third machine; the mechanical bird with flapping wings. To construct a helicoptére was comparatively easy; to make an aëroplane less so; but a mechanical bird offers serious difficulties. All the accounts that have been handed down to us of men flying with wings are very unreliable. It is not sufficient for an inventor to say that he has succeeded in flying, he must show proof of it; and I think it can be safely said that M. Penaud is the first man who has succeeded in making a machine to fly with wings.

M. Marey, whose remarkable researches upon the flight of birds have been published in previous reports, constructed, in 1870, some artificial insects which lifted one third of their own weight. They consisted of a pair of wings attached to a shallow metal basin covered with a thin sheet of rubber, similar to those used by him for recording the movements of the wings of birds. They were placed on the end of a balanced lever which allowed them to rotate in a horizontal direction. Compressed air to work the wings was conveyed to the basin through the upright that supported the lever. These insects

were a step in the right direction, but there still remained two thirds of the weight to be lifted. It was also necessary to make them carry their own motive power and be entirely disconnected from the ground in order to show real flight.

In the latter part of 1871 MM. Penaud and Hureau de Villeneuve, the secretary of the French Society, began to make experiments with mechanical birds propelled by rubber. They called to their aid M. Jobert, a clever workman, who constructed the steel framework required.

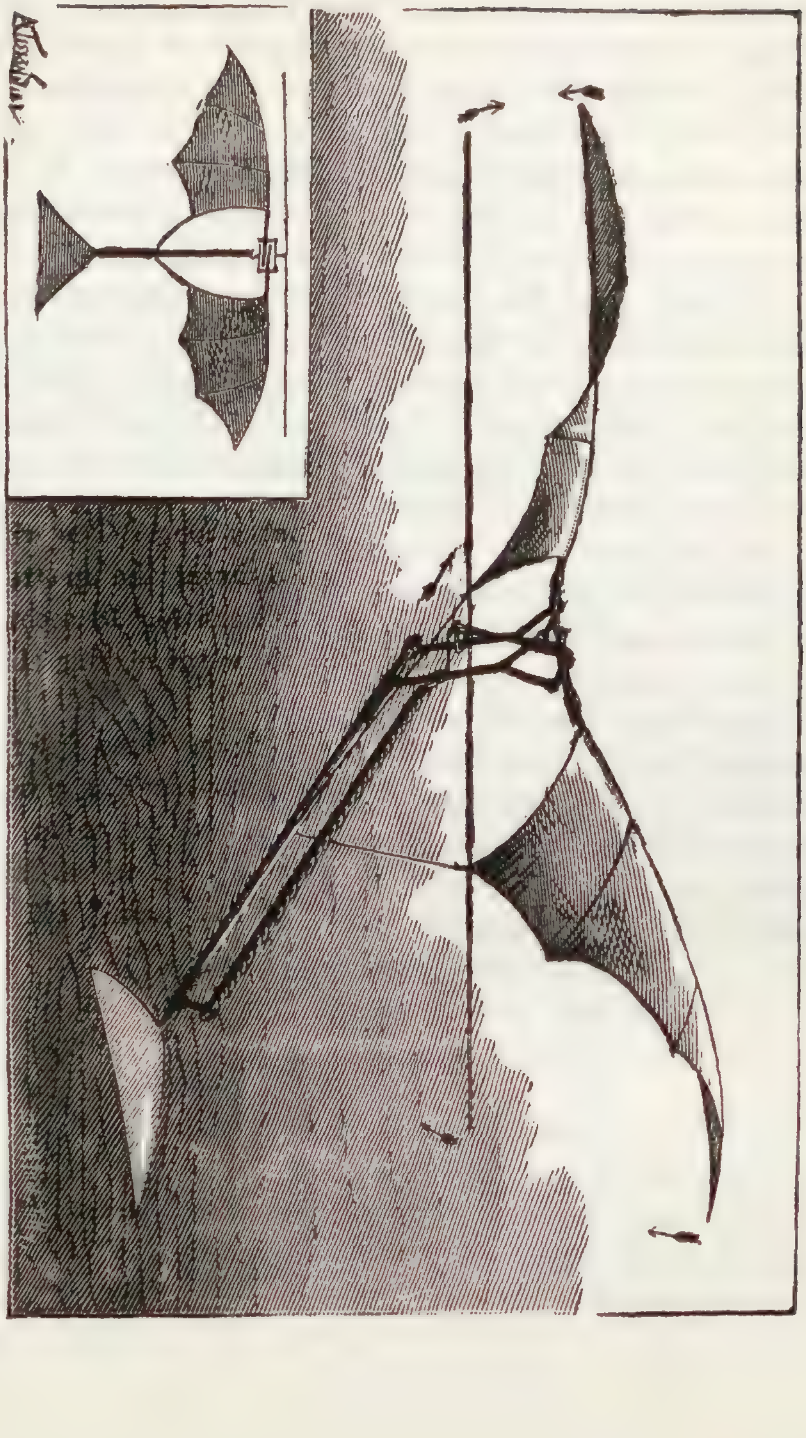
M. de Villeneuve's theory of flight was altogether different from that of M. Penaud's, yet both succeeded in making models to fly. The former after making most elaborate researches into the movements of the shoulder bone of the bat, took it for his model. In his bird the axis of rotation of the wings are oblique, the wings striking downward and forwards. These wings, which are nearly rigid, have a conical movement given them, and the changes in the angles of inclination of their surfaces are entirely due to this movement. M. Penaud on the other hand has constructed his bird after what he calls the "classical theory": viz.—that of Borelli, Cayley, Strauss, Durckeim, and Marey. In his wings the changes in the inclination of the surface is obtained by the elasticity of the sail or back part of the wing, the little sprigs that support it being free to rotate round the rod that forms the front edge. Rubber springs run from the back inner edge of the wings to the centre of the rod which forms the main frame. These springs regulate the movements of the sprigs and give the wing its elasticity, performing a similar function to that of the hind claw of the bat. The torsion and changes in the inclination of the wings are thus regulated by the combined action of the pressure of the air and these springs. The front edge of the wing has a simple up-and-down movement, which the elastic motive power transmits to it through the intervention of a

crank and two rods. When the wing is in its highest position at the end of the upstroke, the rubber springs before mentioned cause it to present its inferior surface forward at an inclination of 15°. Upon the descent of the wing, the resistance of the air causes the outer portion of the wing to twist into a screw shape, the back edge being higher than the front, and thus supports and propels. The inner portion of the wing always remains inclined up, and acts as a kite. In the up-stroke the whole wing supports as a kite, its surface being inclined upwards, the back edge being lower than the front. The wing is thus divided into two distinct parts, one active and the other passive, the outer, which comprises two-thirds of the wing, both supporting and propelling, while the inner portion only supports. The machine is not altogether sustained during the up-stroke so that the down-stroke has to make up for the deficiency.

Fig. 3 is a view of M. Penaud's bird. The wings are shown in the act of descending, the inner portion being inclined forward, and acting as a kite, the outer part being inclined backward, and propelling and supporting. The equilibrium is perfectly maintained by the tail. This model is unable to rise from the ground; but upon being thrown off the hand it descends some 2 feet, and then having acquired velocity flies horizontally for a distance of 50 feet, rising about 8 or 9 feet above the point of departure. The duration of flight is seven seconds. The following are the proportions and weight of the model:—each wing is 16 inches in length, and the total weight is 73 grammes (about 2½oz.), thus divided:—

	Grammes.
The two wings	12
Frame	21
Rubber	28
Tail	12
	73

Fig. 3.



M. de Villeneuve's model, thanks to the peculiar motion of its wings, was able to start direct from the ground, but owing to the small number of strokes only rose to the height of 4 feet, when the spring having run down, it descended, forming a parachute. It possessed a remarkable power of rising, and at each stroke the machine was lifted with great force. M. de Villeneuve has since modified it, so that it will fly horizontally for a distance of 24 feet, at a velocity of 20 miles an hour.

M. Sivel, one of the unfortunate victims of the late fatal balloon ascent, when at Leipsic, saw a little steam bird constructed by an optician of that town. It consisted of a globular boiler that would hold about a gallon, supported upon a tripod. In the top of the boiler was a small cylinder, with a two-inch stroke, which worked two wings 32 inches long. The wings were provided with valves, which opened during the up-stroke. The boiler contained spirits of wine sufficient for 38 seconds. This machine would rise vertically 3ft., the wings making about three beats during the flight.

Flight has thus been accomplished on three different principles, and the practicability of a flying-machine proved. M. Penaud, whose opinion should have great weight, thinks the aëroplane to be the only practicable machine ; but he fears that it will be many years before aërial navigation will be realised. Let us hope not.

CONCLUDING REMARKS.

THIS, our Ninth Report, brings us very nearly to the tenth year of our existence as a Society.

In our short review of the past it will be necessary to allude to the fatal accident of Mr. de Groof, which resulted from his descent from a balloon in an apparatus designed and constructed by himself for purposes of flight.

As this mishap might be taken as evidence of the difficulty, if not impossibility, of accomplishing mechanical flight, a few of the facts may be recorded.

It was stated that in a previous attempt the machine and aëronaut were severed from the balloon at a considerable altitude, and that at his descent he distanced the balloon and reached the ground several fields in advance. Subsequent evidence showed this to be an incorrect report.

At the second ascent, when the machine was detached, the wings were seen to collapse together over head, as if the muscular force of the legs, to which they were connected by cords, was not sufficient to keep them extended, consequently the fall was exceedingly rapid.

The wings measured 37ft. from end to end, so that the leverage was very great. Had they been prevented from folding quite back, by means of suitable stops, the descent might not have proved fatal, though the experiment would have been far from safe for the following reasons :—The area of the wings and

tail, as extended horizontally, was 220 square feet. The weight of the man and machine was 350lbs. If he could not move his wings so as to aid his support, the rate of perpendicular descent would be 1540ft. per minute, being limited to this speed by the resistance of the atmosphere at 1·6lbs. per square foot. 1540ft. per minute is the velocity acquired at the termination of a descent from a height of 11ft., an unsafe distance for an ordinary person to fall, but the feat might be performed by a trained acrobat without damage to himself.

It would appear, therefore, that the arrangement was badly conceived and carried out, without regard to data or principles, and that the position taken by the wings afforded no support. Had they remained extended horizontally the result would have been different, and the descent gradual like that of a parachute.

It has been stated at previous meetings that the Society was desirous of testing the application of the screw to a balloon for the purpose of effecting ascent and descent. This was recognized as a means for prolonging the life of a balloon, and presented the only material improvement of which, in the opinion of the Council, a balloon was to be made capable, while floating in obedience to the direction of the wind, of altering its altitude without parting with gas or ballast.

In a former report the advantages of such an appliance were enumerated.

It was during the time that instructions had been given for the manufacture of a suitable arrangement, that Mr. Bowdler received permission to test an apparatus at the Royal Arsenal, for the propulsion of a balloon by means of a fan or propeller fixed to the car of a balloon. It would not have been necessary to allude to this attempt, but for the fact that the same apparatus combined also a propeller to raise and depress the balloon.

The result was exactly as the initiated would conceive.

It may be stated, however, that the balloon of 60,000 cubic feet was large in proportion to the means employed: Mr. Bowdler raised a doubt of success upon this account.

The first trial was made vertically. The following account is given by Captain C. Orde Brown in the *Popular Science Review*, October, 1874.

"Major Beaumont, Mr. Coxwell, Mr. Bowdler, and a Sergeant of the Royal Engineers entered the car, which was carefully balanced, and the first part of the programme was commenced, the balloon being held captive. Owing to a deficiency of suitable rope the raising was only carried out to the height of about 40ft. instead of 150. The difficulty of ascertaining exactly when a captive balloon is balanced, when even a slight wind is blowing so as to stretch the retaining rope, made the first trial a little doubtful, and after one ascent, apparently due to the working of the propeller, a doubt arose as to the exact balance of the balloon, which might have a tendency to rise and only have been held down by the captive line, which, except at very still moments, was pulled taut by the wind acting on the balloon. It being ascertained, at a still interval, that the balance was good, the vertical gear was worked and the balloon again rose. The rate of ascent was difficult to estimate, it was judged, however, not to exceed 50ft. a minute. A positive indication of the power of the propeller was thus obtained; and it should be noticed that the circumstances, if the rate of ascent only was measured, were rather disadvantageous, for the weight of the line, up to the extent of 40ft., was gradually added to the balloon as it rose. Had the mean rate of ascent and descent been taken this error would be eliminated, for the descent would be favoured by the weight of the rope from 40ft. in length at the maximum height down to nothing at the ground. The balloon was now liberated, not,

however, until Mr. Bowdler's vertical gear had become broken and unable to work. The wind's direction in the meantime had been ascertained to be suitable by sending off a series of small pilot balloons, and the ascent took place. The horizontal gear, however, throughout the entire voyage, failed to give any satisfactory results; even allowing that the effect was perceptible, it is impossible to lay much stress on it. Any force would give a perceptible effect if recorded with sufficient delicacy. There is no use in an insignificant effect unless it can be shown that means exist by which it could be increased sufficiently to bear a reasonable relation to the forces to which is to be opposed, or with which it is expected to be compounded."

Therefore the experiment which the Society had advocated for years, and which it had at length determined to adopt, having proved successful under disadvantageous circumstances when tried by others, the apparatus ordered was countermanded.

When the balloon is used in the future for other purposes than exhibition, perhaps this adjunct may be utilized.

Captain Burnaby, of the Council, has been in the habit of making balloon ascents both by day and night. He recognized the importance of determining the direction he was travelling when out of sight of earth.

The absence of such means in the late war caused mishap to several balloons.

In a recent ascent at the Crystal Palace he states that he obtained the direction by dropping two parachutes, with an interval of time between their liberation, and that by taking the direction of a silken cord which connected the two, he was enabled to verify his course.

During the past year the Authorities at the War Office have been earnest in their inquiries as to the best mode of aërial observation. The cumbersome nature of the apparatus

for manufacturing hydrogen gas, operates against the successful employment of that mode of inflation.

The hot-air balloon of M. Menier and Mr. Simmons afforded to the Authorities some hope that successful ascents might be obtained through such means. The particular experiments may be passed over without further attention in consideration of the fact that the Balloon was proved incapable of inflation even in a very slight breeze.

It was found that the air could not be made sufficiently expansive inside the balloon to counteract the force of wind against the outside surface.

This affords another instance of the mistakes which arise from the enthusiasm of aëronautical inventors, which causes them to draw too favourable inferences from "Parlour Experiments," conducted always under the absence of conditions which attach to real work.

The idea which naturally suggests itself in connexion with this recorded failure, is that the kite might be utilized when the balloon could not be inflated.—*See remarks upon Kites in 1st Annual Report, page 65; and 2nd Annual Report, page 67.*

In the "Concluding Remarks" of the last Report some description of Moy and Shill's Aërial Steamer is given, as it existed at that time.

Several months of close application in subsequent trials resulted in the making of a new engine, and the strengthening of various parts.

The accompanying wood cut is from a Photograph taken in the grounds of the Crystal Palace. The engine which drives the two wheels is contained in a case 27in. by 27½in. by 7½in.; diameter of cylinder 2·125in.; length of stroke 3in.; tube surface 8 square feet. The axle runs right through the steam chest with long bearings, and a tube to keep steam from coming in contact with the axle.

Two eccentrics are formed, each in one piece with the crank pin. The guide rods are made to serve a double purpose. A light cross-head carries the valve rod, and the valve cuts off the steam at half stroke; pressure of steam from 120 to 160 to the square inch.

When this engine was finished and found to work well, it had to be fitted into a frame in order to attach it to the aëroplanes; and a great number of bamboo canes were used in carrying this out. A triangular frame was made, which may be called a tricycle frame. The wheels were all made to go straight forward only, and not to turn a circle. On this frame was built up, about 4ft from the ground, the frame which held the engine and lamps. Other frames extended from this on each side to take the axles of the 6ft. driving wheels. These axles are 3ft. 3in. in length, and 1½in. diameter, made of drawn brass tube, and very light and strong. A front fixed aëroplane was fitted of 50 square feet of surface, and a similarly fixed aëroplane was fitted behind with 64 square feet of surface. Both these fixed planes were set at 10 degrees from the horizontal, and if driven at 35 miles an hour were sufficient to bear up the whole weight of the steamer, which amounted to 214lbs. The driving surface of the revolving aëroplanes amounted to 60 square feet.

This was erected in the Rotunda, in the Crystal Palace grounds; and with this engine and these revolving planes some important experiments were tried to test the truth of the experiments, at Messrs. Penns' Factory, Greenwich. If the old theory was correct, it was expected that the pressure on the planes would only amount to a few ounces per foot: if the new theory was correect the pressure would far exceed that of the old. It turned out, most conclusively, that the old theory was wrong and the new theory right.

At a revolving speed of 20 miles an hour, and with the

pitch or angle of the planes set at 15 degrees, the pressure was exactly one pound to the square foot. And at the same speed of 20 miles an hour, and the angle set at 45 degrees, the pressure was one pound and a half to each square foot of surface. These experiments were very satisfactory, and showed clearly that the inventor was working in the right direction.

After more work, and necessarily more delay, it was determined to try the steamer in the open. It was found that one of the fountains at the Crystal Palace had a path round it of a diameter of nearly 300ft., and it was determined to give it a run round this under steam. A pole was erected in the centre of the fountain, and two cords from the top of the pole to each end of the steamer kept it at one uniform distance from the centre. The gravel had been rolled and steam was got up. The gravel however was too rough: it shook the steamer and offered so much resistance that it had to be abandoned until a smoother road could be obtained. The authorities at the Crystal Palace then kindly consented to lend 8000 square feet of boarding, and it was laid down round the same fountain. More delays, more work, and more patient waiting, with heavy falls of snow on the melancholy looking boards, and weeks of public wonderment as to why those boards were laid down; at length steam was got up, and a good run was made round the fountain, the wing-wheels only acting as drivers. At this experiment a speed of at least 33 miles an hour was required in order to make the steamer leave the ground. But, although it ran on the boards, the friction, and consequently the tractive force was much too heavy. It, however, attained a speed of 12 miles an hour, with plenty of steam to spare, and formed a very pretty sight in the bright sunshine. This was the first time that a machine weighing two hundred weight had ever been driven by it's own motive power by revolving planes imping-ing on the air.

Those used to bicycles and tricycles will know that the latter require an enormous amount of exertion compared to the former, and that three wheels fixed only for forward motion offer a very great resistance to turning a circle.

Had it been possible to place the whole upon a railway the effect sought might have been attained.

This suggestion was offered with respect to an imaginary aërial carriage in our first Annual Report, page 66 (*see* "Concluding Remarks").

While the preparations for these experiments were going on, Mr. Moy determined upon attempting vertical ascent, without the necessity of a previous run.

In the Report of the Aëronautical Exhibition in 1868, drawn up by Mr. Wenham, occur the following sentences:—

"Though we are still without a precise demonstration of the power required for flight in the way that a bird flies, the force to maintain which, in some species, must be very small, yet we have some evidence of the power required to lift a weight in the air by means of vertical screws. By this method it has been demonstrated that 100lb. may be supported by a constant force of about 90,000 foot pounds, or three-horse power.

"Now, in the work of Mr. Stringfellow, the Society has brought out the remarkable fact that a one-horse power engine can be made to weigh only 13lbs.; thus showing the possibility of obtaining flight by the repudiated system of vertical screws, even with the enormous expenditure of power that this plan is known to require."

Viscount D'Amécourt attacked the problem with superposed screw actuated by a small steam engine with aluminium boiler, but as the model was a valuable one it was not set free at the Crystal Palace in 1868. It was stated that it was capable of raising itself to a great height. This was but a toy, however.

In pursuance of Mr. Moy's determination new aëroplane wheels were constructed 12ft. in diameter, and linen planes, carefully stretched, were fitted; the planes revolving in horizontal orbits. These wheels came to grief, not being strong enough; others were made and failed, and the last pair were made of three layers of bent wood, and up to the present time they stand very well. The planes have been set at various angles, but the results have not varied exactly in accordance with those angles, because they do not act successively on undisturbed air.

An experiment was tried with 12 planes to each wheel, the total surface being about 160 feet, when one plane broke with the pressure. It was cut away and it was worked with 11 planes in one wheel and 12 in the other.

The Hon. Secretary, who had hitherto been a constant attendant, having been disabled by a collision occurring on the Crystal Palace line, Captain Greenfield, of the Royal Artillery, one of our Members, very kindly offered to act in his place. This he continued to do for many weeks, and it is by his report that we are enabled to confirm the facts here stated.

Mr. Moy had in the aërial steamer an engine of about three-horse power weighing 80lb.

Would it raise 100lb.—that is, would it raise itself and 20lb. additional?

By carefully weighing and balancing it was found that upon actual experiment the engine was capable of lifting 120lb.

Let us recapitulate the particulars, in order to show how this calculation is arrived at. The piston is 2⅛in. diameter, stroke 3in., revolutions of engine 536 per minute (steam blowing off all the time) revolutions of aëroplane wheels 67 per minute, pressure of steam 140lbs. per square inch, cut off at half-stroke, giving 99,696 foot pounds per minute. This speed gave nearly one pound per square foot of aëroplane.

It must now be very carefully noticed that although these aëroplanes were working in a path of *disturbed* air, yet this result was arrived at, that 3 indicated horses power lifted one hundred weight in round numbers; but suppose this steamer had been large enough to contain an engineer, and that he could so manœuvre as to make it act, in ascending, on *undisturbed* air, the pressure would then have been nearer 2lbs. per square foot, and the engine resistance would then have increased in a like proportion; but he would at once have altered all the angles to a much finer pitch; this would ease the duty again on the engine and give all the lift required.

Upon a subsequent occasion the experiment was repeated in presence of the Duke of Argyll, the Duke of Sutherland, the Earl of Dufferin, Mr. Wright, Mr. Donaldson Hudson, Capt. Greenfield, and Mr. F. W. Brearey, who were satisfied that the experiment, so far as it went, was a complete success.

The members of this Society will read with interest the forgoing account, and will acknowledge that, although our progress has been slow, it is promising, and calculated to awaken the energies of engineers and capitalists.

It was one of the effects of our Exhibition of 1868, that it drew forth and encouraged the energies of such a man as Mr. Moy, who, being then an exhibitor, secured the assistance of Mr. Shill, another exhibitor, for the purpose of working at this difficult problem, to forward which the Aëronautical Exhibition was inaugurated.

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THE FOLLOWING

SPECIFICATIONS OF PATENTS.

No.	*Date.* 1874.	*Subject.*	*Patentee.*
81	Jan. 7.	Improved Apparatus for Navigating the Air	Henric Christian de Vogt.
777	Mar. 3.	Improvements in, and apparatus for Aërial Navigation	Joseph Douglas Ridley.
1144	Apr. 2.	War and Commercial Aërostatic—Hot-air Balloons	Jean Sebastian. Anacharsis Ménier
2808	Aug. 14.	Improvements in Aërial Navigation, and in Apparatus for effecting the same	Thomas Moy.
2821	Aug. 15.	Improvements in the manufacture of Light Gases, and in the method of Inflating Balloons therewith (for Military & other purposes), & in Machinery and Apparatus for such purposes, and for Directing, Guiding, Propelling, and Managing such Balloons	Isham Baggs.
3132	Sep. 12.	Improvements in the Construction of Balloons, and in Apparatus applicable thereto	Joseph Simmons.
3177	Sep. 12.	Means and Apparatus for Aërial Navigation...	Frederick Hime.
3371	Oct. 2.	An improved mode of and Apparatus for propelling vessels through air or water	Henry Whiteside Cook.
3831	Nov. 6.	Improvements in Balloons and in the method and Apparatus for inflating the same	Alexander Watt.
3996	Nov. 21.	Improvements in Navigable Balloons, applicable also as a mechanical and philosophical toy (communicated by Stanislas Ludovic Brion)	Edwin Powley Alexander.

Tenth Annual Report

OF THE

AËRONAUTICAL SOCIETY

OF

GREAT BRITAIN.

FOR THE YEAR 1875.

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THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN.

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Ladies may become Associates upon the same terms.

Tenth Annual Report

OF THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN,

FOR THE YEAR 1875,

Containing an Account of the Proceedings, and a Selection from the Papers and Communications received by the Society during the year, with concluding Remarks upon the present state of the Science.

THE ANNUAL MEETING of this Society was held on Wednesday, the 23rd June, 1875, at Eight o'clock, in the Rooms of the Society of Arts, John Street, Adelphi. JAMES GLAISHER, Esq., F.R.S., presided over a fairly-well attended Meeting. The audience included several ladies. Particular interest attached to the present assembly in view of a Paper by Mr. Moy, on his recent invention of "The Aërial Steamer."

Mr. F. W. BREAREY, the Secretary, began the proceedings by announcing that Mr. Glaisher would take the chair.

The CHAIRMAN, on assuming that position, said:—Ladies and Gentlemen, it has fallen to my lot on several occasions, as you are aware, in the absence of the President to occupy this chair, and on each occasion I have had some points of progress to indicate in regard to the labours of the past year, and on the present occasion there is perhaps as much to speak of as in any one of those years. I have now merely to speak of the intricate and

difficult problems which so many good men have been endeavouring to solve. Two of our own Members, Messrs. Moy and Shill, have been spending I should be sorry to say how much money, but, more than that, they have been spending I do not know how much time; but I am not ignorant of the very high ability with which they have devoted themselves from time to time to the construction of a new steam engine on their own pattern. They have achieved a degree of success which I could scarcely have expected last year, and they have now produced a machine which has lifted vertically a weight of 120lbs. This fact is so important and so pregnant with our future success that I will not now longer occupy your time with speaking of it, as we have Mr. Moy himself here to explain to you what he has done and what he still expects this engine will be able to do.

Another prominent feature in the past year's work is the sad calamity abroad. It was the case of gentlemen devoting themselves not to the mere pursuit of novelty, but using the balloon with a view of increasing our knowledge, and in that pursuit they lost their lives. It was a sad calamity, but I will not dwell longer upon it now, as I thought you would expect from me some particulars of the journey which ended so fatally, and I have been furnished with many facts by my friend, M. Tissandier, and I shall devote some time this evening to speaking upon them; therefore I will not now claim your time longer, but will ask Mr. Moy to give us, as fully as he thinks proper, the results he has achieved.

Mr. Moy read a Paper on "Aëronautical Progress."

After indulging in a diatribe upon the inclination of man to destroy birds in the name of sport, Mr. Moy read as follows:—

When I first began seriously to contemplate mechanical flight, now nearly 30 years ago, I had very little hope of

imitating any of the numerous living examples which I saw continually around. Like a great many more I thought, for some years, of the "displacement theory." A certain amount of gas of small specific gravity, enclosed in a certain bag of a certain shape, and propelled by some suitable power, and the best that man could do would be accomplished. This was my idea from 1847 to 1859; but in this period the steam engine had made some rapid strides, and engines of something less than one ton to the horse-power had been made, and I began to think that steam might after all be made available to propel aëroplanes, and that Henson and Stringfellow might have succeeded if they had had more power, with a more effective imitation of nature.

In 1865 the celebrated "Reign of Law," by His Grace the Duke of Argyll, was read by many with an interest which was entirely due to its merits, and after much thought upon the subject in that year, I discovered that an inclined plane might be driven at any reasonable speed, whether fast or slow, with the same power, provided the front edge was rigid and sharp, and that the angle was made only sufficient to prevent falling at the desired speed. From that moment I abandoned gas.

After much thought and time we had the Exhibition of this Society, in 1868, at the Crystal Palace, which gave a fresh impetus to the activity of my mind upon the subject, and many plans were formed, half matured, and thrown aside for more hopeful ideas. Amidst all this hard work it was always kept in mind that the motive-power required remodelling, and having looked about in vain for a suitable steam engine, I thought out the steam engine which is well known as the "Moy and Shill engine," and in which the great, cumbrous, separate boiler is entirely done away with.

Having got the motive-power, the next step was to apply

it; and here the road was anything but a Royal one. I have heard Members of this Society urge the *necessity* of density and gravitation to the accomplishment of aërial flight; but I can assure those Members that there is nothing easier than to retain density or weight, and that the law of gravitation *fully* recognizes every pound of the weight. The great trouble is to get rid of sufficient density, and so to reduce thereby the effects of gravitation, that the motive-power will be strong enough to overcome that amount of gravitation which cannot be got rid of. To do this it is useless to have recourse to displacement, as this at once makes the resistance so formidable that speed becomes impossible.

It would take too much time to trace the successive steps which have been made. In 1869 I had settled the main features of the aërial machine, or, as we will in future call it, the "aërial steamer."

Diagrams handed round.

The diagrams, Figures 1, 2, and 4 (now handed about) show how the revolving and fixed aëroplanes are disposed, with the engines in the centre of the cabin. One pair of wheels were to be driven by one engine forward, and the other pair by the after engine, and the four wheels so geared together that one engine was at the half-stroke, while the other was at a dead point, in order to insure continuous motion.

A great deal of time and money was expended in carrying this out. The novel patterns for this engine were made from my drawings by my partner, Mr. Shill. The castings were of gun-metal; diameter of cylinder 2 inches; length of stroke 2 inches; steam at 100lbs. to the square inch was cut off at one-eighth of the stroke.

The revolving planes were fitted into hoops made of pine, 6ft. diameter, wire in tension being used instead of spokes, like the well-known bicycle phantom wheels. The outer planes

were so adjusted that as they rose upward in their successive revolutions they received an upward pressure on their under surfaces, and for about 200 degrees of the revolution the pressure was on the reverse side of the planes, and thus the effective action was without any wasted effort.

Numerous reconstructions took place in the engines, valve-gear, framing, wheels, &c., until it was thought advisable to design another engine with greater power. This was commenced last Autumn, and I think I may say that it has turned out to be the best engine for its weight that has been yet constructed.

Mr. Moy then proceeded to give particulars of the experiments which had taken place at the Crystal Palace, but, as they were embodied in the "Concluding Remarks" of the Annual Report for 1874, this part of Mr. Moy's Paper may be omitted. His Paper continues as follows :—

Every real inventor has to deal with the irrepressible objector and the self-styled "practical man," and I may say that the stupidity exhibited by the objectors to this steam engine for the last three years is something marvellous. I need not repeat their objections here. It is simply a steam engine, and in it steam is raised rapidly, used economically and effectually, and weight can be reduced to a minimum when required; space is also greatly reduced as well as first cost; and if those advantages are not sufficient I should like to know what will satisfy the fastidious requirements of the irrepressibles. If any one still doubts the merits of this steam engine, let him produce a 3-horse engine which will lift in air 50 per cent. more than its own weight; a 6-horse engine which will lift in air 100 per cent. more than its own weight; or a 100 horse-power engine which will lift 40 hundred weights, and then, *if* he can do that, I surrender.

Now, having cleared the ground, I come to practical application.

I propose to build an engine of 100 horse-power, ascertain exactly what it will do by dynamometer, then build the aërial steamer with sufficient surface, cabin accommodation, and steering apparatus, to make it rise vertically from the ground, and when clear of the trees, &c., to alter the angles of the aëroplane wheels and aëroplanes so as to travel in any required direction.

You will notice in the diagram that the revolving aëroplane wheels are perfectly horizontal. This gives a vertical effect so as to raise it directly from the ground. Having done this the orbit of the wheels can be altered so as to cause the impulse to take a diagonal direction, when the steamer will travel in the direction of the arrow. If higher speed is required the aëroplane wheels are made to assume a more vertical orbit, and the pitch of the planes is increased, the fixed planes coming more and more into action as the speed is increased; and when it is required to slacken speed, the angle of the orbit of the aëroplane wheels is caused to approach again to the horizontal position, and the speed can be reduced as desired.

But you will say this is all very fine talking, but what surfaces will you employ, and what materials will you be able to adopt? This has all been thought out, and tables made from data already obtained, which show that, for a really practical machine, very little else than steel and gun-metal will be required, so that it will be more of the style of a stag-beetle than a butterfly.

It is quite as easy to get 50lbs. to the square foot of surface as it is to get one pound; and when we get 50lbs. we can use metallic surfaces, and the space travelled is passed through at a much higher speed. The revolving aëroplanes also to a large machine can be made far more efficient than those of a model, and comparatively lighter.

If any of you were to attempt to model a windmill, on the scale of half-an-inch to the foot, with its delicate controlling apparatus, you would find it simply impossible to keep to scale; yet the real working windmill is comparatively very easy to make, and so it is with the model aërial steamer. Many important parts are obliged to be entirely omitted in the model which would be absolutely necessary in the large steamer, and could, on a large scale, be made comparatively light; and thus it is that we can calmly contemplate making a large aërial steamer suitable to carry passengers with comfort, and to condense all our steam and use the condensed water again in the boiler, to make the engines on the compound principle, and the various arrangements for steering and controlling the steamer all complete and handy, while in models these must be omitted because they would be too delicate.

One is met with such singular remarks on this subject that a few more particulars would not be out of place here. The body, or, to use a ship-building phrase, the "hull" of the aërial steamer will be 70ft. in length, 8ft. beam, and 8ft. in depth, well trussed to give longitudinal stiffness. This will be of metal, and a considerable portion of the roof and sides will be cellular, having about an inch of space between the outer and inner surfaces. The outer surface will, of course, be in contact with the air, the inner surface will be in contact with the air in the cabins. The space between these two surfaces is a receptacle for the exhaust steam. Every one knows that when the exhaust steam is let into this space it cannot continue steam; it must condense, and in doing so it parts with its latent heat, which is, of course, taken away by these metallic surfaces. The water trickling down the sides will be passed again into the steam generator to be again converted into steam, and thus render unnecessary any great store of water, and the heat parted with by the condensed steam will warm the cabins.

There is no reason why the cabins should not be made comfortable, and at whatever speed the steamer travels the aëronauts would never be exposed to the current of air. All the controlling gear will be trained into the cabin, and windows will be fitted in the sides and bottom of the cabins to enable the aëronauts to travel by observation. The steering to the right or left will be effected by means of the driving wheels, the angles of which can be altered at will, and the amount of elevation can be effected by the fixed and steering planes.

In illustration of the vertical movement, I have here a little model belonging to our active and worthy Secretary, Mr. Brearey, which, by the rotation of four wings in contrary directions, raises itself. It does not carry up its motive-power, as the power is exerted in winding up the spring, but it serves to illustrate the action of the rotating inclined surfaces.

Models are very pretty things to illustrate a Lecture, and, when well made, are very effective, but they entail an enormous amount of trouble and delicate manipulation, and the speed of rotation is necessarily so great, that gyroscopic action takes place and interferes very greatly with the intended effect; and I must confess to a strong feeling of dislike to the finnicking work. The model at the Crystal Palace is 25ft. wide, and is quite small enough to my mind, and I should have had great pleasure in having it here to-night and working it before you; but as the wheels are 12ft. in diameter, and of course require a doorway of that size to admit them, the steamer could not be brought here.

In concluding a Paper on "Aëronautical Progress" it would be very unfair to omit to mention the interesting and thoughtful labours of Mr. Wenham, whose Papers, read before this Society, and accounts of experiments made by him, have been of a most interesting and useful character, and whose steady firmness in the ultimate success of this Society is beyond all praise.

There is a class of objectors to aërial navigation who are very fond of *Time*. "Yes, my good fellow, 50 or 100 years hence it may be done, but *not in our* time, oh no! bye-and-bye, but not now." These people seem to see a great merit in distance and reverence delay, and, for the life of me, I cannot see where the advantage lies. I believe in "work" and in making use of the present time, and, as I told you in 1868 so I tell you now, that thought, work, and money can and will do it.

On the conclusion of the Paper

The CHAIRMAN said there was one question of the Paper which Mr. Moy could make clearer, and that was, whether the 120lbs. weight had been determined by a Salter's Balance?

Mr. MOY said they found the Salter's Balance was interfered with by the motion of the piston. They weighed the machine carefully and found it weighed 186lbs. They then put little levers, one on each side, with weights, to take off all above 120lbs. The machine then rose an inch-and-a-half from the floor.

The CHAIRMAN: Then it was determined by actual experiment? because the Paper led us to think it was based upon calculation only. It is evident that it is an absolute fact that it did rise this height from the ground.

Mr. MOY: The Duke of Argyll and the other noblemen and gentlemen present were quite satisfied with it.

Mr. BROWN: It does not appear to me whether the engine, boiler, and framework completely rose. Did the whole machine go up?

Mr. MOY: The engine with its generator and frame weighed 80lbs. It lifted 120lbs.

Mr. BROWN: I suppose the whole went up?

Mr. MOY said 80lbs. was the weight of the engine, there-

fore the engine lifted itself and 40lbs. besides. Balanced levers lifted 66lbs. of the whole 186lbs.

Mr. Wenham remarked that he had little to say on the general subject, but as Mr. Moy was flying a light engine he could give him a little information on the subject. He (Mr. Wenham) had prepared a condenser. The exhausted steam was passed into a chamber where 15 cubic feet of air was sufficient to condense one foot of steam. The condenser was very light, the walls being made of tin plate. Condensation was obtained simply by mixing steam with air, in a way that seemed never to have been practically done before. Mr. Moy, as a practical man, would see that such a condenser could be made of the thinnest metal; tin plate in fact.

Captain Greenfield, R.A.: It may be interesting to gentlemen here to have the evidence of an outsider on this matter. In Mr. Brearey's absence during the spring, when he was suffering from injuries received in a railway accident, I officiated for him for a short time as Secretary of the Society, and on the strength of that basis Messrs. Moy, Shill, and Childs very kindly invited me to these experiments on Monday last before the Duke of Argyll, the Duke of Sutherland, the Earl of Dufferin, and several others. I took notes of what I observed, and recorded the revolutions and the amount of work done. I should first describe how the engine was arranged. It rested on the floor on its base. The engine was resting on a very narrow base against the framework of the machine, and the two ends, that is to say the spindles of the wing wheels, were supported by little levers, the arms of which were of equal length, and each one was weighted to about 33lbs. on each side, making 66lbs. on both. 66lbs. weight was thus taken off the engine by those little levers. The work to be done was to raise—no matter whether one inch, one foot, or one mile—the work was to raise the whole engine, then weighing

120lbs. One of the wing-planes having been broken previously to the experiment there remained on the one side only eleven wing-planes against twelve on the other side, consequently the wing on one side was unable to do its work completely; therefore I found the complete and perfect wing rose much more than did the imperfect one. The complete one rose on one occasion six inches, lifting the whole body of the apparatus a distance of two inches from the ground; therefore the work done was that 120lbs. was lifted by the apparatus within itself two inches from the ground at the very least. On the other side it was lifted six inches.

The CHAIRMAN: Vertically?

Captain GREENFIELD: Vertically. This was done with the assistance of 66lbs. at the extremities, without any other power than the revolution of the wings in the air; but that was in still air, not in moving air. There was no draught at the time the machine was in motion. I have reason, from what I saw then, to believe that, by a slight re-adjustment, the engines ought to be able to do more than that. In a space of 94-5 seconds I made a calculation of 99000 odd pounds of pressure.

Mr. MOY: Foot-pounds?

Captain GREENFIELD: Foot pounds of course. The wings being unequal in operating there was a good deal of oscillating motion. One wing was unable to rise as much as the other, but at last, as I say, one wing rose six inches and the other rose two inches, giving a mean of three or four inches. (Applause.)

The CHAIRMAN: I am sorry I was unable to be present last week to see those experiments. I now rise to ask you to give a vote of thanks to Mr. Moy for his Paper. As I observed in my opening remarks Mr. Moy has spared neither time nor money, but the results have been what you have heard. The remarks by Capt. GREENFIELD on these experiments are exceedingly valuable. I have now to ask you to give your heartiest

thanks to Mr. Moy and your best wishes for the future. May his hopes be realized in further experiments.

The thanks of the Meeting were accorded to Mr. Moy.

The CHAIRMAN then read the following Paper, contributed by himself, on the

DEATH OF CROCÉ-SPINELLI AND SIVEL.

On the morning of Thursday, 15th of April, 1875, at 11.35, the balloon, *Zenith*, left the Gas Works of La Villette, with Messrs. TISSANDIER, CROCÉ-SPINELLI, and SIVEL in the car. Three small balloons, filled with a mixture of air and oxygen in the proportion of 70 to 100, were fastened to the hoop. From the lower part of each depended a tube of caoutchouc, which passed through a wash-bottle filled with an aromatic liquid. This apparatus was intended to supply the voyagers, when in the upper regions of the atmosphere, with the necessary amount of oxygen for maintaining life. An aspirator, filled with essence of petroleum, which would not solidify owing to the fall of temperature, was suspended outside the car. It was to be set up vertically at the height of about 10,000ft., for the purpose of injecting air into the tubes of potassium intended for the determination of the carbonic acid.

Sivel had fixed within reach of his hand some bags of ballast, which would empty of themselves on cutting the string which held them. He had fixed underneath the car a thick mattress of straw to deaden the shock of descent. Crocé-Spinelli had brought with him his spectroscope, so frequently employed in the preceding voyage of the *Zenith*. From the ropes of the car were suspended two Aneroid barometers, which had been verified, previously to starting, under the air-pump, the first giving readings corresponding to heights from 0 to 13,000ft., the second readings from 13,000 to 29,500ft. Near to these instruments was suspended a thermometer of red alcohol for

for the measures of the lowest temperatures, graduated down to 22° Fahrenheit, and a minimum and maximum thermometer which, by means of an endless cord fastened to the valve, could be introduced into the interior of the balloon, for the purpose of determining the temperature of the gas; also, in a sealed box and carefully packed in sawdust, were eight barometric test-tubes, intended to furnish, after the descent of the balloon, an exact record of the greatest height attained. Charts, compasses, and printed question-papers, to be dropped from the car, completed the scientific *matériel* of the expedition.

The balloon ascended at the rate of 6 or 7ft. per second, decreasing gradually till it reached 11,500ft., after which it rose rapidly to 16,500ft., under the action of a brilliant sun and by the discharge of ballast. Sivel during this time was occupied with the anchor-rope, and in taking precautions for landing. Scarcely had 1,000ft. been reached when he joyfully exclaimed, "We are well on our way, my friends. I am very well pleased." A short time after, gazing at the swelling balloon above the car, he said, "Look at the *Zenith*, how well filled she is, how well she looks."

Crocé-Spinelli said, "Now Tissandier: now for the aspirator and the carbonic acid!" and M. Tissandier prepared his apparatus for injecting 70 litres of air into the tubes of potassium at a height of from 13,000ft. to 20,000ft.; but these tubes, which he had not strength at the last moment to secure in the padded box, must in the descent have been broken.

At the height of 10,500ft. gas escaped with force from the open neck of the balloon. The odour was perceptible, but neither Sivel nor Tissandier felt inconvenience from it, but it is important to notice the following lines which were found written in the note-book of Crocé-Spinelli:—"11h. 57m.; barometer nearly 19·68in.; temperature + 2°. Slight pain in the ears. A little oppressed. It is the gas."

It should be stated that the *Zenith* was not fully inflated, so that a large space was left for dilatation.

At 13,000ft. the sun was hot, the sky resplendent, and with numerous cirrus clouds extending to the horizon.

At 14,000ft. they began to breathe oxygen, not because there was any necessity for its use, but only to convince themselves that the apparatus was in working order.

At the height of 23,000ft., at 1h. 20m., Sivel inhaled the mixture of air and oxygen. He was suffering from oppression, and this cordial invigorated him. At this height he wrote in his note-book, "I am breathing oxygen. The effect is excellent."

At the same height Sivel, who possessed great physical strength and a sanguine temperament became drowsy and slightly pale. He would not, however, suffer himself to be overcome. Drawing himself up with an expression of firmness he made Tissandier empty the liquid contained in the aspirator after the experiment, and in order to ascend higher he threw out ballast. Sivel, the year before, had ascended to the height of 23,000ft. with Crocé-Spinelli. He wished this year to ascend to 30,000ft.

Crocé-Spinelli had for some time been occupied with the spectroscope. He was in excellent spirits, and had just exclaimed, "There is a complete absence of the lines of the vapour of water." Having uttered these words he had applied himself to the resumption of observations with so much vigour that he asked M. Tissandier to write in his note-book the results of the readings of the barometer and thermometer.

In the course of the rapid ascent, in the midst of so many occupations, it had been difficult to find time for physiological observations. These it was intended to make in the higher regions of the air, and the aëronauts were reserving for them all their strength, never suspecting the fatal *dénouement* which

was about to stay their efforts. The following physiological observations were, however, made :—

Time. h. m.	Altitude. feet.			
12.48 ...	10,157 ...	Tissandier...	110 pulsations in a minute.	
1.3 ...	10,767 ...	Crocé	120	,, ,,
1.5 ...	10,767 ...	Sivel.........	155	,, ,,

During the ascent up to 23,000ft. meteorological observations were taken regularly. They indicated a gradual diminution of temperature up to 10,000ft. an increase from 10,500ft. to 12,100ft., and, lastly, a gradual diminution from 13,300ft. up to 23,000ft., and above.

Table giving the complete results of the Readings.

Time. h. m.	Altitude. feet.	Temperature. °
11.30		57·2
...	1,194	51·8
...	2,598	46·4
11.40	4,157	46·4
...	6,562	44·6
...	10,499	33·8
...	11,482	34·7
12.15	12,132	35·6
...	13,451	32·0
..	14,393	32·0
...	15,098	32·0
12.51	15,419	32·0
...	17,092	26·6
...	17,092	23·0
...	17,388	23·0
1.5	18,372	23·0
...	19,029	23·0
...	21,981	17·6
1.20	22,965	14·0
..	24,227	12·2
...	26,246	unknown

For the first time the interior temperature of a balloon

has been determined, and the results which have been obtained are of much interest. Sivel had excellently arranged the cord for introducing the thermometer into the interior of the balloon, and Crocé-Spinelli twice made the experiment by means of the apparatus.

The temperature indicated by the thermometer was 66° in the centre, and 72° near the valve, whilst floating at the height of from 15,000ft. to 16,500ft., while the temperature of the surrounding air was 32°. At 17,500ft. the interior temperature of the balloon in the centre was 63°, whilst the external air was 23°. At the time of the catastrophe the thermometer was inside the balloon, and it was found there uninjured after the descent. It had risen to the temperature of 73°.

I now proceed to give in M. Tissandier's own words his account of the fatal result of the voyage.

"At 23,000ft. we were standing up in the car. Sivel, who had given up for a moment is re-invigorated; Crocé-Spinelli is motionless in front of me. 'How beautiful is the cirrus' he remarks, and indeed it was a sublime spectacle which was offered to our sight—cirrus clouds of different forms, some long and others slightly rounded, formed around us a circle of silvery white.

"I felt stupefied and frozen. I wished to put on my fur gloves, but, without being conscious of it, the action of taking them from my pocket necessitated an effort that I could no longer make.

"At the height of 23,000ft. I made entries in my note-book mechanically. I copy, verbatim, the following lines which were written by me, although I have no very distinct remembrance of doing so. They are traced in a hardly legible manner by a hand trembling with cold :—

"'**My hands are frozen. I am all right. We are all all right.**

Fog in the horizon, with little rounded cirrus. We are ascending. Crocé pants. He inhales oxygen. Sivel closes his eyes; Crocé also closes his eyes. I empty the aspirator. Temperature 14° Fahrenheit at 1h. 20m.; barometer reading 12·6in. Sivel is drowsy at 1h. 25m.; temperature 12·2°; barometer reading 11·8in. Sivel throws out ballast.' These last words are hardly readable.

"Sivel, indeed, who had remained for some time pensive and immovable, at times closing his eyes, had just remembered, doubtless, that he wished to ascend beyond the present height. He rouses himself and turns towards me and asks, 'What is the pressure'? I reply, '11·8in.' 'We have plenty of ballast, shall we throw some out?' he asks. I reply, 'do as you like.' He turns towards Crocé and asks him the same question. Crosé makes with his head an energetie sign in the affirmative.

"In the car were at least five bags of ballast; nearly as many were suspended by fine cords outside. These, I ought to add, were not quite filled. Sivel certainly could have stated their weight, but I can give no estimate myself.

"Sivel seized his knife and cut successively three cords, and the three bags emptied themselves and we ascended rapidly. The last remembrance of this ascent which remains clear to me relates to a moment earlier. Crocé-Spinelli was seated, holding in one hand the wash-bottle of oxygen gas. His head was slightly inclined, and he seemed oppressed. I had still strength to tap the Aneroid barometer to facilitate the movement of the needle. Sivel had just raised his hand towards the sky, as if pointing to the upper regions of the atmosphere. As for myself I remained perfectly still, without suspecting that I had perhaps already lost the power of moving. About the height of 25,000ft. the condition of stupefaction which ensues is extraordinary. The mind and body weaken by degrees and imperceptibly, without consciousness of it. No suffering is

experienced; on the contrary, an inner joy is felt like an irradiation from the surrounding flood of light. One becomes indifferent. One thinks no more of the perilous position or of danger. One ascends and is happy to ascend. The vertigo of the upper regions is not an idle word, but, so far as I can judge from my personal impressions, vertigo appears at the last moment; it immediately precedes annihilation, sudden, unexpected, and irresistible.

"When Sivel cut away the bags of ballast at the height of about 24,000ft., that is to say under a pressure of 11·8in., which is the last number written in my book, I seem to remember that he was sitting at the bottom of the car and nearly in the same position as Crocé-Spinelli. For my part I was in the angle of the car, thanks to which support I was able to hold up, but I soon felt too weak even to turn my head to look at my companions.

"Soon I wished to take hold of the tube of oxygen, but it was impossible to raise my arm; my mind, nevertheless, was quite clear.

"I still kept a watch on the barometer. My eyes were fixed upon the needle, which soon arrived at the figure indicating a pressure of 11·4in., then it passed to 11in., and even further.

"I wished to exclaim, 'We are 8000 metres high,' but my tongue was as it were paralyzed. All at once I closed my eyes, and sinking down inert became insensible. This was about 1h. 30m.

"At 2h. 8m. I awoke for a moment and found the balloon rapidly descending. I was able to cut away a bag of ballast to check the speed and write in my note-book the following lines, which I copy:—

"'We are descending. Temperature 3° (Fahrenheit). I throw out ballast. Barometer 12·4in. We are descending.

Sivel and Crocé still in a fainting state at the bottom of the car. Descending very rapidly.'

"Hardly had I written these lines when a kind of trembling seized me, and I fell back weakened again. There was a violent wind from below upward, denoting a very rapid descent. Some minutes after I felt myself shaken by the arm, and I recognised Crocé, who had revived. 'Throw out ballast,' he said to me: 'We are descending;' but I could hardly open my eyes, and did not see whether Sivel was awake.

"I call to mind that Crocé unfastened the aspirator, which he threw overboard, and then he threw out ballast, rugs, &c.

"All this is an extremely confused remembrance, quickly extinguished, for again I fell back inert, more completely than before, and it seemed to me that I was dying.

"What happened? It is certain that the balloon, relieved of a great weight of ballast, at once ascended to the higher regions.

"About 3h. 30m. I opened my eyes again. I felt dreadfully giddy and oppressed, but gradually came to myself. The balloon was descending with frightful speed and making great oscillations. I crept along on my knees, and I pulled Sivel and Crocé by the arm. 'Sivel! Crocé!' I exclaimed, 'Wake up!' My two companions were huddled up motionless in the car, covered by their cloaks. I collected all my strength and endeavoured to raise them up. Sivel's face was black, his eyes dull, and his mouth was open and full of blood. Crocé's eyes were half-closed and his mouth was bloody.

"To relate what happened afterwards is quite impossible. I felt a frightful wind. We were still 9,700ft. high. There remained in the car two bags of ballast, which I threw out. I was drawing near the earth. I looked for my knife to cut the small rope which held the anchor, but could not find it. I was

like a madman and continued to call 'Sivel! Sivel!' By good fortune I was able to put my hand upon my knife and detach the anchor at the right moment. The shock on coming to the ground was dreadful. The balloon seemed as if it were being flattened. I thought it was going to remain where it had fallen, but the wind was high and it was dragged across fields, the anchor not catching. The bodies of my unfortunate friends were shaken about in the car, and I thought every moment they would be jerked out. At length, however, I seized the valve-line, and the gas soon escaped from the balloon, which lodged against a tree. It was then four o'clock.

"On stepping out I was seized with a feverish attack, and sunk down and thought for a moment that I was going to join my friends in the next world: but I came to. I found the bodies of my friends cold and stiff. I had them put under shelter in an adjacent barn.

"The descent of the *Zenith* took place in the plains near Ciron (Indre), 155 miles from Paris, as the crow flies. According to the question-papers dropped from the car, and sent to the Office of the French Society of Aërial Navigation by those who picked them up, I feel certain that the *Zenith* did not deviate from a straight course, that the wind blew in a straight line, and that its direction was constant up to a height of 8,000 metres. The velocity of the air certainly was greater in the upper regions of the atmosphere than on the ground.

"The question-papers did not take less than half-an-hour to descend from the height of 7,000 metres to the ground. A paper dropped mechanically by me at 3h. 30m., at the moment of my second awakening, and spotted with blood from a slight cut which I gave my hand before fainting for the first time, was caught whilst floating in the atmosphere, 35 minutes after the balloon came down.

"Having given the history of the ascent, I come to the

two important questions which have so much engaged the attention of the scientific world and the public, viz. :—'The maximum height attained by the *Zenith*, and the cause of the death of Crocé-Spinelli and Sivel.'

"A reply to the first question has since been given by the opening of the barometric tubes, invented by M. Janssen, and which had been previously employed by Sivel and Crocé-Spinelli in their ascent to 7,300 metres on 22nd March, 1874.

"The tubes taken up in the *Zenith* were examined in the Physical Laboratory of the Sorbonne, by the assistance of MM. Berthelot, Jamin, and Hervé Mangon. The tubes were placed under the air-pump, together with a barometer, the air being exhausted so as gradually to drive the column of mercury into the curved extremity of the tubes to the position it occupied when we attained the greatest elevation. One tube had been broken, several had been injured or had worked badly, but there were two, the march of which had been regular. These furnished concordant results and indicated that the least pressure was from 10·4in. to 10·3in., which indicates a maximum height of from 28,000ft. to 28,200ft.

"It appears to me certain that the death of my unhappy friends was caused by the rarefaction of the atmosphere. It is possible to support, for a short time, the effects of this rarefaction, but it is difficult to submit to its continued action for nearly two consecutive hours. Our sojourn in the upper regions was continued longer than in any preceding high ascent. I may also add that the particular dry air might have contributed to exercise a fatal influence.

"It will be asked to what cause I owe my safety. I owe my life probably to my individual temperament, essentially lymphatic, perhaps to my complete swoon having caused a kind of arrest of the respiratory functions. I was fasting at the moment of departure, a circumstance which I at first

thought peculiar to myself, but I have since had proof that, whether Sivel had eaten or not, Crocé had, like myself, scarcely any food in the stomach.

"With one exception the few preceding high ascents are far removed from this altitude. Gay Lussac, in 1804, attained 7,004 metres; Robertson, in 1803, 7,400 metres; Barral and Bixio, in 1852, 7,016 metres; Welsh, the same year, 6,990 metres. All these voyages, it will be seen, have been limited to heights between 7,000 and 7,400 metres, which I believe should be considered as the limits of the respirable atmosphere.

"Our friend and master, Mr. Glaisher, in 1862, ascended to the height of 8,838 metres. He then suddenly became insensible and nearly lost his life, and, he has since said, supposed himself about to die. The height which he believed himself to have further attained, 11,000 metres, appears to be very doubtful, as it is only determined by an algebraic proportion deduced from the speed of the balloon in its ascent and descent. This *savant* assumes these velocities to have been constant during the time of his unconsciousness, whilst they may have varied and the speed of the ascent have become *nil*. I may add that Mr. Glaisher had made similar expeditions, that he had trained himself little by little, and that it is certain that his organization had become accustomed to the influence of the rarefaction of the air, which had endued him with peculiar faculties for the performance of these voyages.

"I am persuaded that Crocé-Spinelli and Sivel would be still living, notwithstanding their long stay in the upper regions, if they had been able to breath oxygen. Like myself, no doubt they suddenly lost the power of moving, and the abduction-tubes escaped from their paralyzed hands."

M. Tissandier adds that he learns, from information afforded by the Mayor of Cournenin, that the aspirator fell close to a woman sitting on the grass with her two children,

the noise produced by the shock being very considerable. A rug and a padded box, intended for the potassium tubes, were also found near.

After the reading of the Paper

The CHAIRMAN said—A few remarks may justly be expected from me. It seems to me most strange that three gentlemen, of different ages and different physique, should simultaneously become exhausted. I have been at the height at which this occurred several times and never felt any inconvenience. The question then arises—Why did these gentlemen die? What was the cause of their death? When I was about six miles high I was insensible for want of oxygen, but when we came down I recovered again. No blood could have entered into their mouths if they had died from rarefaction of the air. It is the occurrence of the blood which seems to me to present so much difficulty. I cannot but think that, through indiscreetly throwing out a great weight of ballast, the balloon must have ascended like an arrow from a bow. The balloon would then either burst or the gas escape, and it might have been that they were seated within the influence of the escaping gas, and this caused the blood to come. However I cannot satisfy myself as to what was the cause of death. I am sure we shall all feel admiration for those gentlemen who, from no light motive, made this ascent to increase human knowledge. With respect to the remark of M. Tissandier, that the extreme height of 11,000 metres was obtained by an algebraic proportion. I would add that two other independent determinations led to the same result. I never had any feeling of joy at great heights: it was one of intense agony at five or six miles; beyond that the death is painless. I had no pain after we were six miles high. Though the death itself is painless, nature seems, at certain heights, to say "go

back, you are dying." I had none of that ecstatic feeling experienced by the French gentlemen. I am quite ignorant of it. Directly we approached four miles from the earth I felt pain. I am sure I may now say that this Society feels very keenly the loss of those two gentlemen who expired in the pursuit of science. A subscription has, I believe, been made for their widows and families.

Captain Burnaby gave some notes of experiments at the Crystal Palace. These, he said, had reference to an instrument he had invented some time previously for the purpose of ascertaining the direction the balloon was going when floating in a space above the clouds, and more particularly at night. As many gentlemen present knew, he had made ascents at night when it was almost impossible to get a line. The compass indicated East, West, North, and South, but the earth was hid from their view, the clouds were going the same way as they were, and, for anything they knew, they might be going towards France or Germany. He had, therefore, thought of employing two small parachutes to indicate the direction in which the balloon was travelling. The parachutes could be made of silk. They would have magnesium wire in their cars, and must be attached the one to the other by a long silken thread. This, in its turn, is fastened to a reel in the car of the balloon. On dropping one parachute it would at first fall on the motion of the balloon, but the attraction of the earth would gradually make the parachute descend. In a few seconds he would let fall a second parachute. This would act in a similar manner; and then, by drawing an imaginary line in the mind's eye from the first to the second parachute, the aëronaut could discover the direction in which he was travelling. This he believed, was most important with respect to warfare, and particularly in respect of postal balloons sent out of a fortress at night; otherwise they would not know whether they were

going into the country of the enemy or that of friends. By this invention they would be able to ascertain the course of the balloon, and to know whether they should descend or continue their course. This was a subject which he believed had never been worked, and he had thought it of sufficient importance to bring it before the Society.

Mr. WENHAM asked whether the parachutes could be drawn up again.

Captain BURNABY: Yes; that is the advantage, because you have a silk cord connecting the two parachutes and connected with the car by a reel, so that you cannot lose the parachutes.

The CHAIRMAN said whenever he had been above the clouds and lost sight of the earth he could always determine the direction of motion by means of the hanging grapnel rope. If the balloon was standing still the grapnel was vertical, if moving at all it was out of the vertical, and by looking at the compass he always knew in which direction he was moving. That was by daylight; but in night ascents he had still seen the rope. Captain Burnaby, who had been with him, must have remarked that the rope could be seen at night.

Captain BURNABY said they might be able to see it, but he had known cases when he could not see his hand before him. It had been so dark that he could see nothing. He had had the opportunity of talking on this subject with several of the men who went up from Paris during the siege.

The CHAIRMAN: They were sailors and inexperienced men, with the exception of M. De Fonveille and two or three others.

Captain BURNABY thought this did not meet the case. The balloon ascents were mostly made by day because there were no means of knowing the direction at night.

Mr. WENHAM said he could not exactly see on what principle an anchor, suspended from a balloon, should deviate

from the perpendicular. Captain Burnaby's parachutes, if left at rest, would, after a time, partake of the motion of the car; but while the parachute was being quickly raised or lowered it would have a tendency to fall perpendicularly, and the balloon at the time traversing in a direction away from the line of gravitation taken by the parachute, a sensible inclination of the suspending cord would indicate the direction in which the balloon was travelling.

The CHAIRMAN remarked that the grapnel always followed the balloon.

Captain BURNABY: At times you cannot see the anchor at all.

The CHAIRMAN: I have been in the car of a balloon when we could not see the balloon itself.

Captain BURNABY: That is what I make a great point of. That is the time when it is impossible for the aëronaut to know the direction in which he is going, but this invention of mine will enable him to do so.

The CHAIRMAN observed that Captain Burnaby spoke from practical knowledge, and that the Meeting was much obliged to him for giving the result of his experience.

A vote of thanks was given to Captain Burnaby.

M. MENIER read a Paper, in French, on Experiments in Guiding Balloons. Several model balloons, inflated and furnished with the steering apparatus, attached to the balloon in the form of small sails, were exhibited in the room.

M. MENIER said the system of aërial navigation he proposed was based upon the employment of hot air with accessory surfaces placed on each side of the balloon. A hot-air balloon was tried at Woolwich for military purposes. It was true the balloon had met with an accident, but it was also quite certain that, on the 16th October, it did rise and lifted a weight of 1,700lbs. With some change in the balloon it was his opinion it would answer for military purposes. It was this

system of the hot-air balloon that had given him the idea of a plan of aërial navigation and of propelling and steering balloons. He was not quite sure he understood Mr. Moy, but if his object was simply to support one man in the air by means of machinery, he was afraid that was not the intention of aërial navigation. Aërial navigation, to be useful, must be able to take the produce of a place from that place to the place of consumption. The balloon itself seems to offer that facility, and it was for that reason he had endeavoured to steer the balloon; but what were the means of steering it? The first was the power of ascent and descent communicated to the balloon. The second was the resistance offered by the air to any surface passing through it with speed. One presented the difference of velocity and the second the difference of atmosphere. If we employed the power of ascending and descending, and the resistance of atmosphere, we might probably be able to steer the balloon. He commended his two inventions to this Society, and should be glad if they could give him some support.

Mr. Moy said this plan of driving balloons was an old acquaintance of his; it was in an old number of the Mechanics' Magazine.

M. Menier exhibited two balloons fitted with steering apparatus and one without. The one unfitted with sails ascended vertically. One of the others with sails set took a direction to the right, the other to the left. The experiment was therefere, in a limited space and an undisturbed atmosphere, successful.*

Mr. Moy said he had seen a Mechanics' Magazine of 1824,

* A Model upon this principle was shown by Mr. Heath at the Aëronautical Society's Exhibition at the Crystal Palace, in 1868. It was thus described in the Catalogue—"Model of a Balloon with a ring or belt attached, which, in ascent or descent, is placed in an inclined position relative to the axis of the Balloon . . . ." (Ed.)

in which the same idea was described. If they threw out ballast to get ascensive power, and let out gas to get descensive power, they would require so much of both that they would find the process a most expensive one. A small amount of steam would drive a screw with greater power.

Mr. WENHAM expressed his belief that Sir George Cayley was the original inventor of this plan.

Captain BURNABY asked if this machine could go against a wind blowing at fifteen miles an hour. In that room, where there was no draught, these fans made but a very slight deflection from the regular course. If there was so small a deviation now what would there be with the wind at fifteen miles an hour?

M. MENIER said he made no pretensions at a first trial to go against the wind. He supposed the propelling and steering of the balloon must commence at one point and go on at another time to another. All he now professed to show was that it might be possible to do that which had, as yet, been done by nobody. It was not his purpose to show that this balloon could go against the wind, but he supposed learned men, as the Members of that Society were, would think it something if he showed them the balloon, without requiring it to go against the wind. They might calculate from the ascent, descent, and resistance what power could be given to the balloon.

Mr. MOY: You would save time if you would give figures.

M. MENIER said he was not prepared to give figures. When it was said he should be obliged to throw out ballast and lose gas, he must explain that he only used gas for the purpose of experiment, and that he intended his apparatus to be applied to the hot-air balloon only.

Captain BURNABY expressed his disapproval of the hot-air balloon on the ground of the difficulty of inflation and the danger on touching the ground. The danger of the balloon

catching fire was also to be considered. For military purposes the difficulty of inflating the hot-air balloon made it practically useless.

The CHAIRMAN could hardly believe the balloon could be guided in a strong wind, but he was sure, at the same time, they would all heartily give M. Menier their thanks.

A vote of thanks was given to M. Menier.

The CHAIRMAN, in adjourning the Meeting, expressed an earnest hope that when they met again Mr. Moy would have taken another step in the direction in which he had commenced.

On the motion of Captain Burnaby the thanks of the Meeting were given to the Chairman, and the Meeting separated.

The following Paper, though not read before the Society, is inserted as being a popularized exponent of ideas upon the subject of aëronautics, and because the views put forth therein agree with the experiments and theories of several well-known Members of the Aëronautical Society.

ANGUS AND MACK ON THE AIR PATH.

Two friends, ANGUS and MACK by name, were sitting together one evening lately by the fireside, and their talk was of those in the *Alert* and the *Discovery*, who would then be taking their lonely winter stations apart among the ice, far north of even the usual summer tracks of man.

In all the region there, there would soon be only one warm spot, carefully enclosed; only one spot where there would be light, and that light never but making dark shadows on the wall; and with room outside for many a gale between there and the remote point southward where daylight, like a tide that had ebbed, would so long be lingering. And over the trackless waste intervening, riven, and wind swept, and drifted, what manner of messenger could bring word home to tell how it was with them when the long night was closing in.

There seemed only the air path; but as the friends were not of one mind regarding its availability for man, and began to argue: and, as the inertia of doubt in the one was needed to discover the force of experimental belief in the other, we shall let each speak for himself, in his own way.

ANGUS.—A fleein machine do ye say? that'll never be.

MACK.—What for no? I'm sure ye've only to look out o' doors to see no ane only, but whole flocks o' them; ye need'na gang farther than the sparrow for an instance.

ANGUS.—Aye, aye, but ye're no a sparrow.

MACK.—I ken that, but that's nae argument.

ANGUS.—Aweel then, we'll try an mak it ane; bulk for

bulk we'll grant that the sparrow an you are equal in weight, but wi' you, a good deal o' your bulk is made up o' legs, showing clearly where nature has designed much o' your strength should lie: whereas she has gathered the beef o' the sparrow about its shouthers tae work the wings, leaving it wi' scarcely ony legs at a'.

MACK.—Let the beef bulk where it may, the head will mak a' the difference.

ANGUS.—Ay, but no ony o' the difference between rinnin and fleein: though the head may sometimes feel light enough for fleein if it wer'na for the weight o' the body.

MACK.—I'm sure ye might jist as weel say it has made nane o' the difference between rinnin and ridin in a railway-train.

ANGUS.—Noo MACK, be reasonable; jist waff your hand to and fro this way, an tell me if ye feel onything like what ye could rin your railway-train on.

MACK.—Hoots man, your hand is ower numb to feel onything that would stop short o' hurting. The sparrow waffs his hands tae mair purpose than you can dae.

ANGUS.—I dinna see hoo granting that can help ye much. You yersell would flee left-handed ye ken.

MACK.—Its no the hands but the head that's in question. When a' things are ready, the hands maun hae naething to dae but turn a handle, or open a bit tap noo and again.

ANGUS.—What's the handle to dae? and what's to come oot o' the taps? win?

MACK.—Maybe ay, and maybe yes. I expect there will be nae want o' win' about them, onyway.

ANGUS.—Jokin apart though, if there's to be so little for ye to dae when ye're up fleein, what do ye mean should dae the wark o' lifting ye and driving ye? for ye'll be nae mair

able to get quit o' your gravity when rising in the air than when gaun up stairs.

MACK.—We'll speak o' the driving power when we ken better what power 's required: mair than likely it'll be maist needed at the start; in which case, a store o' ready prepared force might do much; and some birds appear to exert themselves sae little once they are up, it's not unreasonable to hope that we may learn a way to tak it easy too. Ae thing at least is clear, we shall have naething but physical forces to deal with, and there's surely mathematics among us sufficient to find out a' that's wanted.

ANGUS.—Ay; and if mathematics happen to get a haud o' the wrang end o' the string, they can jist talk as much nonsense as onybody. I dinna think ye're ready for them yet, and ye'll hae a gude deal o' open air wark before ye can gie them the fundamentals to operate on, and while they're operating then, ye'll maybe be fleein, and they'll finish in time to tell ye whether the principles ye're fleein on are fleein anes or no.

MACK.—I wish I was in the way o' giving them a chance.

ANGUS.—Can ye no try figures for yoursell?

MACK.—Do ye want to gie me a sair head?

ANGUS.—Not for the world; get somebody wi' a harder head than your ain to risk the sairness, for I doubt without figures ye'll get badly on; and I doubt the birds'll no help ye much, else man would hae got the cue frae them lang syne.

MACK.—Oh; but man lang syne did'na ken sae much as we ken noo. Why, hoo lang was man in the habit of bilin water before he could see mair in the steam than that, when the pot lid was tight the hot water cam belchin frae the spout, and when the spout was at last cut off, and the pot became a close biler, Papin biled banes in't to make jelly; and to have hinted then about puttin the biler on to wheels, and rinnin 't

on rails at railway speed would have appeared as like delirium as to some noo appears this talk o' man ever fleein; and folks wer'na then ignorant folk either, though they wer'na sae weel up in some things as oorsells, ye ken. Further, hoo lang was man blawn about by the win' before he found out that win' had weight; and noo that he has found that out he seems no farther forward with it than was Papin with his biler bilin banes.

ANGUS.—Ou ay; but we're gaun to gie the thing a lift forward, ye ken. The folks lang syne did'na ken ony better, and the folks noo might hear us; and it's ower soon to draw comments upon oorsells, for we ha'na ta'en root yet, and, indeed, where to plant oursells either for fleein or for lookin on is no jist clear yet. It's you that's gaun up, ye ken.

MACK.—Oh, I'll no be feared.

ANGUS.—But where does the weight o' the air come in? I dinna jist see.

MACK.—Wherever it's in motion; and no till then.

ANGUS.—Then nae motion nae weight, is that it?

MACK.—No, but nae pressure nae resistance frae the weight o' air that the pressure has to put in motion. The simple weight is raither mair than an ounce and a quarter to a cubic foot; the resistance is according to the pressure in motion, so if ye can impose the pressure ye'll be a' right for the resistance. Oh ye may jist as weel blaw on your fingers as waff your hands—ye'll no find onything oot that way.

ANGUS.—Weel, but what resistance do ye look for frae an ounce and a quarter o' caller air?

MACK.—If the ounce and a quarter were there alone, ye would hae to be gentle wi't, but as it canna gang oot o' the road without displacing its neighbours, who are as heavy as itsell, it'll suffer compression in itsell equal to the resistance that the whole of them offer to displacement, or to that much o't that they're slow in yielding to.

ANGUS.—And hoo far out will it claim neighbours among the surrounding ounce and quarters?

MACK.—Till among them they can balance the pressure; but hoo far out I raither think 'll depend on the time allowed; and that'll depend on whether the imposed pressure is seeking mere passing support, or is bodily displacing air frae the front to the rear of the body; and, in the latter case, much will depend on the length o' the body frae front to rear, and on its form. I hae na got the length o' kenning mair than that. Could your hand no tell us something? Waff't again.

ANGUS.—Ou, ay, it tells me there's something gey saft in the business. Its no the hand, and ye're trying your head.

MACK.—Na, na; what ye're calling saftness the learned call mobility, and there's no a bird among them a' could flee a yard if their road was na as substantial as ony railroad, and a good deal smoother.

ANGUS.—But look ye: we'll say the cubic foot o' air gives only a square foot o' surface for the wing to rest on, noo what weight o' body do ye propose to allow for that square foot o' wing surface?

MACK.—Weel ye see, if the wings are to be a' in motion, and none o' them mere floats, we might put between twa and three pounds on a square foot o' wing. But dinna mistak, for when fleein there'll no be twa or three pounds on the square foot o' air surface, and as little will there be that weight on the wings that press the air surface. D'ye understand?

ANGUS.—Indeed I dinna—but gang on, I'll follow. What do ye do wi' the weight, if neither the wings nor the air carry it?

MACK.—Oh, but they do carry it—that is, the air carries it—and the wings jist spread it out like on the air. D'ye see noo?

ANGUS.—No, I'm quite blin'.

MACK.—Weel then, we'll suppose that the weight has been got up, and is in fleein motion, say on a horizontal line, which implies that there's a force at wark opposing the action o' gravity, by mechanically developing in the air, come upon as much resistance upward, as the weight has natural tendency downward. Noo, as the weight is not allowed to fall any, this tendency in it will be aye at zero, aye ready, but never beginning, ony mair than if it were supported by this table.

ANGUS.—Yes; but hoo do ye spread out the weight? for it seems to me that wherever the body is, there also will be the weight; and ye surely dinna think o' spreading oot the body.

MACK.—Oh no. We need only to spread oot force equal to the force that the attraction o' the earth would uniformly develop in the matter o' the body. If we stop the attraction from producing motion in the body, by transferring equivalent motion mechanically to the sustaining wings, we hae the weight o' body still entire, but without the freedom o' motion earthward to make it dynamically sensible. And, as simple weight is only the force o' attraction between earth and body made sensible in pressure—further, as the force o' attraction for a given weight is uniformly equal in value in equal spaces o' time, and we balance it by oor wing force, this wing force need, in amount, be nae mair than the amount o' force o' attraction between earth and body for that time—so that the faster the wings can travel horizontally in a given time, the less need be the pressure on them to balance the uniform force o' the earth's attraction in the body; for they'll then get the sustaining resistance o' a greater surface o' air than when travelling slower: and as there is only a definite amount o' resistance wanted, the greater surface will have less to bear per square foot. Noo do ye understand?

ANGUS.—Honestly, no yet; for I canna get ower the

weight upon the table; it has nae energy o' motion earthward, ony mair than your fleein weight, but the table has to bear it a' the same. Jist tak' the weight into your hand, and tell me what difference would your hand feel if it were fleein wi' the weight.

MACK.—Hoots, man. If with the body in your hand ye support your hand on the air and move it horizontally, so, the extension o' the air support will be equivalent to an extention o' the hand, and as the force o' attraction has dynamic value only in relation to time, we have the force due to one second of time distributed along the extended hand support for one second—that is, we produce or develop in the supporting air in one second o' time, dynamic resistance equal to what would be developed in the weight if baith hand and air were removed. A weight resting on the hand is but a dead weight, it's the wings mak a' the difference. If Sir Isaac Newton's apple had had wings in motion when it left the twig, he would ne'er hae got the hint that has made us a' wise men; but we might instead hae been soon a' fleein, and wha would hae bothered themsells wi' railroads then?

ANGUS.—Weel noo. I canna say whether ye be right or wrang; but hoo are ye gaun to get up to put it to the proof?

MACK.—Wi' a rin on the grund and strength o' will, maybe; or maybe we'll drap ower some brae-head to get a lang slide on the air to gather speed; but I ha'na jist come to that yet, and canna say.

ANGUS.—But what do ye mean to do about feathers?

MACK.—I ha'na got so far as them yet. I'm still only seeking for first principles.

ANGUS.—Ye're no sae far advanced then as some I read o' in the Society's last Report; they hae got the length o' legs and india-rubber.

MACK.—Ah, but that's no on oor side o' the water.

The legs ye read o' are French anes. Oor folk are using steam, and they can dae without a biler.

Angus.—Ay; and I see they're proposing to use a separater, to get the oxygen oot o' the air to save coals.

Mack.—Weel, weel, the mind 'll no sit still; it maun aye be on the march.

Angus.—I notice besides, that ane o' the Members, writin about the wave o' expansion on the rear edge of a floating wing, shaves gey near perpetual motion.

Mack.—Angus, what do ye call the motion o' the moon round the earth? or o' the earth round its pole?

Angus.—Oh, if ye're gaun to be sair aboot it we'll let it gang; it's as feasible onyway as the motive force which anither Member derived from his stick when he hit his fleein frames as if he had been hitting a fleein cuddy; but there noo, ye needna speak. I'll no say anither word on that score: and I dare say a stick's as gude a first principle as onything ye've named yet; there, that'll dae noo, I've dune. There's plenty else we can speak o' withoot fa'in oot aboot it: ye ken, I'm no speaking in ill-humour. Ye hae had your say aboot the weight o' air, but it has yet to be made clear what use is to be made o' the 15lbs. to the square inch natural pressure when fleein.

Mack.—Nane mair than to keep the weight what it is. If ye gang up to where this pressure has its beginning ye'll find the weight has its beginning there too, for the natural pressure is jist the value o' the weight o' column frae tap to bottom—naething else.

Angus.—Hoo can that be, if a cubic foot o't, at the bottom where the 15lbs. is, weighs only one ounce and a quarter?

Mack.—Jist because ye weigh it by itsell, and leave the column abune oot o' the count when weighing, for it has only determined the density at the bottom. But ye're surely no needin to be told a' that, ye were lang enough at the schule.

Angus.—Oh, man, there was only Greek and Latin at the schule where I was, sae ye maun hae patience wi' me jist ae step farther. If the bottom cubic foot o' air is bearing a pressure o' 15lbs. to the square inch of surface, surely ye have to start this pressure into motion when ye move the air.

Mack.—Not if it be motion o' displacement only, because the compressive and expansive forces are in balance in the air, and in mere displacement baith gang wi't, leaving in the form o' sensible resistance only the inertia o' the simple weight o' the volume put in motion. If it wer'na for that what would become o' us when the win' blaws?

Angus.—We would hae to gang wi't tae, and pray it would'na tak us near the water.

Mack.—Ye maun understand that the 15lbs. pressure becomes sensible force only when ye form a vacuum; the expansive force then wi' the compressive force as an abutment on ae side, and naething on the ither, acts in the direction o' the naething, like a spring of 15lbs. power.

Angus.—And I suppose, in the case o' a partial vacuum, it would enter wi' only proportionate strength.

Mack.—It would; and as the 15lbs. balanced elastic force is for the surface o' the earth, and the density, and with it the elastic force, increases every foot ye descend below the surface; if ye can bring pressure equal say to the weight o' one foot extra of air column, to bear by way o' helping the compressive force, the expansive force at that point will be for the moment driven back upon itself by the mechanically applied force, say from a bird's wing, until it intensifies to balance the applied mechanical pressure plus the natural compressive force; but this must be quickly done to prevent the mobility o' the air come upon from acting outwards in free displacement.

Angus.—Man, do ye no think some graphic experiments

wi' reds and blues ye ken, to distinguish ae kind o' pressure frae anither would help us baith?

MACK.—We hae had the real thing practising before oor een lang enough: rowing birds and soaring birds, birds that whirr and flee straight, and birds that flee in a' manner o' ither ways. What could we hae more graphic than that?

ANGUS.—Hoots ay, man, we ken a' that; and there's nae end tae the poetry that's been written about it, but can ye no put the mechanics o't in form. Hae ye ever tried?

MACK.—Oh, the wee anes are ower quick, and the big anes are ower far awa. There's a fellow countryman o' oor ain, hooever, has scrapet their banes and measured the knots and gullies in their shouther joints. And round aboot his garden got to be famous for the evidences o' devotion to science, a' in distress. Neer a cat need want its dinner there if it could eat a sparrow wi' its wings clippet. Ye see he wanted to find out the fleein feathers.

ANGUS.—Could figures no hae served his turn as weel, without ill using the puir things?

MACK.—Ou ay; he used figures tae, at least he used ane—the figure 8—but it was only for a symbol; and what was a wheen sparrows, compared wi' a question that was to revolutionize the world.

ANGUS.—It was deil's wark at the best; and ye ken the sparrows are no the deil's. I hope there are nae mair philosophers wi' shears.

MACK.—I dinna ken; but a knife can do some things better than shears.

ANGUS.—Some things waur than wing clipping d'ye mean?

MACK.—Ay; but it was na by ony countryman o' oor ain. He wanted tae ken whether the beef o' birds had extra pith in it or no; sae he strappit down a living bird upon a

table, and cut awa till he got the shouther muscles a' bare, and loosened frae ane anither, and the elbow-joint, for convenience, disarticulated. The muscle twitched when it got an electric shock, and he measured the livin' force in the twitch by means o' weights tied tae ane o' the sair ends.

ANGUS.—Weel, a' I can say is, if ye lay the foundations o' the science in the blood and suffering o' the innocent, the Lord'll never prosper ye. Man, I jist wish my anger were o' mair consequence—but let's change the subject. Let's talk aboot the win' that ye'll hae to lay your foundations on. Will ye flee against the win' or wi' it?

MACK.—If the win' be strong it will be easier tae gang wi't: if it be very strong there'll be nae alternative wi' a numb machine but gang.

ANGUS.—Tae let it blaw ye alang like?

MACK.—No indeed: if to begin wi', ye were tae gang slower than the win', ye might lose control; but that would depend greatly on the form o' your machine; and if at the same rate as the win', ye would be practically in a calm, and the back o' the wing would be nae stiffer to bear ye than would air at rest, for the momentum o' air in motion can become sensible only in pressure o' resistance to it. If going faster than the win', that would be equivalent tae starting frae a point o' rest in calm air, and the velocity additional tae that o' the win' would be the working velocity for support.

ANGUS.—But ye dinna mean tae say ye learnt that frae the birds?

MACK.—No, for it's seldom their business in life requires them to travel in sic a hurry, or as far as the length o' a gale.

ANGUS.—But what aboot the ocean birds, don't they aye keep head to windward?

MACK.—Aye when they're no wanting tae gang the ither way. But, speaking o' the win', jist look to-morrow at the

reek rising frae some lum tap, where there's a column o't the width o' the pot. If the win' be moving horizontally at say aboot the same easy rate as the column o' smoke vertically, ye'll see the column bending, without much losing the form that the pot has given it, and rise some height before the win' takes it horizontally awa; that's owing tae the weight in the rising column being, at the start, equal in inertia tae the weight o' the win' in contact wi't there, else would the win' blaw straight through the column.

ANGUS.—Windy observations on a lum tap; but I'm listening. I'll look up in the morning.

MACK.—Oh, but I hae some windier anes. In the late gale, in crossing the West high bridge, I found mysell at one part unexpectedly in a dead calm, though my head and shouthers were above the wall. Resting my hand on the wall-top, there was still nae win'; but on projecting my hand beyond the outer edge I found that the win' stopped by the wall was being deflected upwards, and that the momentum o' its weight in upward motion was forming an arch o' resistance tae the pressure abune the wall; and for some height abune the wall the arch could be felt as plainly as if formed o' spouting water.

ANGUS.—But ye surely dinna mean to mak ony sic commotion when ye're fleeing?

MACK.—No, for we'll be fleeing an the brigg was na.

ANGUS.—Is that windy arch onything like the wave o' expansion we had the remark aboot?

MACK.—No, for the arch was formed by two currents o' weight in cross motion; whereas the wave will act by the expansive energy of the compressed volume o' air, in much the same manner as the expansive force acts in filling up a partial vacuum. Ye'll take notice, hooever, that the wave energy has nae concern wi' the 15lbs. natural force, but only wi' the

sensible pressure distinct from it, and due to the compression o' the sustaining volume by the wing plane; consequently the energy o' the reaction and its velocity will be correspondingly less than when a vacuum has to be filled up.

Angus.—Ay, but stop a bit. I've read somewhere that sound travels on a wave moving at the rate o' mair than a thousand feet a second, and ye'll maybe have noticed that when a big gun is fired the windows in the neighbourhood rattle in their casements aboot the same instant that ye hear the gun. Noo, hoo does your wave o' expansion stand in relation tae that ane? It seems to me they belang tae the same family.

Mack.—I've nae doubt they dae; but ye'll observe that it is not necessary for the shaking o' the window that the weight o' the whole body o' air between it and the gun is blawn against it. Drap a smooth pebble into still water, and the waves that gang circling oot frae the spot will explain my meaning: any light things floating in the way will show that they are waves o' oscillation only. In the case o' the air wave caused by the gun, the window arrests the oscillation and consequently shakes. Ye'll observe, further, that there's a succession o' waves frae the centre where ye drapped the pebble, because the trough in the rear o' the wave is below the original mean level o' the water, and so develops a succeeding wave; and similarly in air, where in place o' the height and hollow o' the water wave form we hae rarefaction and compression in rapid alternation.

Angus.—But you don't mean tae say there's naething but oscillation in the track o' the shot?

Mack.—No; there's local disturbance there, jist as there is in the track o' a bird's wing.

Angus.—Then oot frae the local disturbance o' a bird's wing we may look for oscillation I suppose?

Mack.—Reasonably we may, as the sound o' a flapping

sail travels as fast as that o' a gun, big or little, only it thins out sooner, because the weight o' the oscillation is less. If the disturbance o' the displacement under a bird's wing wer'na local mainly, so as to be governed by the common law o' gravity, but had its displacement-motion propagated wi' the velocity o' the waves o' sound, this motion and the motion o' the wing would na correspond at a'.

ANGUS.—Weel, wi' as many waves o' oscillation as there'll be in say a big flock o' American pigeons, hoo dae the bottom birds get on at a'?

MACK.—I dinna think it would be safe for me tae gang ony farther in explanation. There's Tyndall, ye ken.

ANGUS.—Oh, man, dinna fear, Tyndall would na mind ye; but ye'll be safer on the fleein track wi' the local disturbance; ye'll there be as wise as himsell maybe. As for mysell, I wish ye were talking aboot things that the mind could form some image o'. Let's hear something mair aboot the handles and the taps that are tae keep a' the energies ye speak o' in fleein order.

MACK.—Ay, but we hae na got the length o' needing handles yet. We maun first arrange the fleein order before we can make a picture o't.

ANGUS.—Aweel, I'll wait till the picture's ready; and, mind ye, let's hae a man in't this time. But surely ye hae formed some mechanical notion o' hoo tae put the energies tae use, for withoot something o' that sort they can be o' nae mair profit than the moral excellencies wi' naebody tae claim them. Is your machine tae be lang or short, round or flat? or hae ye the bird in your ee for a pattern? Puir things, they'll hae a sair time o't for a while when ye get up among them.

MACK.—Weel, the machine and the bird'll baith flee, and they'll baith mak the road that's tae sustain them by compressing, in the same fashion, a layer o' air tae the density

suited tae their weight, in much the same way as a garden roller, in motion, compresses saft grund; but I don't know that the bird and the machine'll hae onything else in common.

ANGUS.—That parallel has some weight onyway, and deserves consideration. Od' man; I never thought o' a garden roller in that connexion before. I'm afraid, hooever, ye'll no be able tae carry your parallel very far into the question.

MACK.—Oh it's no necessary. Ye said ye could best understand what your mind could form some image o', and I'm no against images mysell. A short or narrow roller on saft ground would only mak a rut track for itsell; whereas a long roller o' the same diameter and weight, by spreading that weight ower a wider path, would gang easy on the surface.

ANGUS.—But what in the air answers to the rut in the grund?

MACK.—Oh, there can be nae ruts in the air road: ye would come doon through the road if ye were tae narrow your footing there.

ANGUS.—Not I, ye may be sure o' that: ye'll neer get me tae gang up tae roll the win'.

MACK.—Wha's thinking o' rolling the win'? Sliding's the word.

ANGUS.—What made ye speak o' a roller then?

MACK.—For the sake o' the image, and because the question o' lateral extension o' surface applies tae the air in fleein, as weel as tae the earth in rolling, and fleein's sliding, as sure as rolling's circling round a centre; and as the air road's no made tae hand, and the air has tae be come on wi' the suddenness o' a surprise, to be pressed on lightly and awa before it has had time tae get oot o' the way, it seems mair than likely that, for the light short tread, the sliding planes answering to the wings o' a bird will be narrow measured in the direction o' flight, and laterally long, and the length laterally shall be the measure o' the width o' road.

ANGUS.—But if ye mak your wing planes sae narrow what'll become o' your wave o' expansion that's tae dae sae much on the rear edge?

MACK.—Oh, the wing planes'll no be narrower than the wings of the bigger birds, and the wave, I expect, acts in them, though I've never seen't.

ANGUS.—But the lateral extension'll mak them sae supple that I fear the wave would be at a loss in places tae ken the rear edge frae the fore ane.

MACK.—Oh, we can clip the ends if we see ony uncertainty o' that sort, and jist gang a wee bit faster tae mak up for what's cut aff.

ANGUS.—But ye'll need sae many o' them, ane coming hard on the heels o' anither I suppose, that I dinna see hoo the air can be come on wi' the suddenness o' the surprise ye speak o' wi' ony mair o' them than the first ane. A bird has only twa, ye ken, ane on each side.

MACK.—Man, the machine a'thegither 'll be sae unlike a bird, ye can hardly reason frae the ane tae the ither.

ANGUS.—Where does the difference begin? Is it at the strings and whalebone or at the man?

MACK.—Wi' a thing that has na had a beginning yet it would be hard tae say.

ANGUS.—Aweel, its clear that I'm no tae get the picture o' the thing the night. A lum tap, and a stane brigg, and a garden roller: there's nae uncertainty aboot them onyway; and I suppose ye'll be haudin at them till, wi' the light short tread, the touch and awa, ye gae aff to whustle among the albatrosses. Lets ken when ye're a' ready, for I can hooray weel.

Mack's mind got oot o' harness when Angus left, and ran awa tae play wi' some fancies that had been patiently waiting for its leisure. The night was na cauld, but it was dark; and

frae the black darkness spread oot below cam up the surging sound o' an ocean o' billows on the march, before a dour droning gale.

Owerhead were clouds no far aff. In front, so near that in the dark they seemed within reach o' the hand, were forms vague and undefinable in continuous whizzing motion, and the whizzing sound made known tae him that there were similar forms behind.

It was like travelling in a dream, and hoo far he might hae travelled, or hoo lang, he was na thinking, when the door opened, and in cam Mrs. Mack tae ask him if he did na think it was time tae gang to bed. While she was yet speaking his mind crept back intae its harness again, for the dream was at an end.

II—A FEW WEEKS LATER.

Angus.—Weel, Mack, ye're busy as usual I see, aye sowing and harrowing in; but I forget if I've ever seen ye reaping. However, this is Mr. Howie, who believes in the air road, I think even mair stoutly than yoursell, for he sees nae difficulties.

Mack.—I'm glad to see ye baith. Draw in chairs and sit ye down.

Angus.—Ah, weel, we'll jist sit down on the edge o' them, for we hae na lang to stop. I would hae waited till we had mair time, but Howie would'na. He thinks that the hour has come, and the world's jist waiting for the man, and he's in a hurry tae get forward,

Howie.—Nothing of the sort, Angus. I thought the hurry was your own.

Angus.—Oh, it's ower soon to blaw the horn yet, is't? My mistake has na come far. My arm felt as if linking wi' a blawn blether when ye were yarning about air dynamics in my lug on the road here; but let that pass. The twa o' ye at it noo. Howie wants to ken if ye're doing onything at present in the fleein line.

Howie.—Well, I do; but I hope the answer wont be quite so blunt as the inquiry.

Angus.—Hout man, dinna fear. Mack's maist as fou o' the thing as ye are yoursell.

Mack.—It's a quiet founess then, for I've done naething since you were here last.

Angus.—Howie'll beat ye in the race then; and its a grand prize that'll be given tae the winner: a monument at least.

Mack.—There'll be mony a ane ending the race, as ye call it, that did na begin it; and monument or no, there'll maybe be mony a ane giving the world a thankless gift o' half their days that canna weel afford it, no tae speak o' the sair heart that may be left wi' them when they see naebody taking notice o' their absence at the winning post. Better they had taken their imaginations tae the grundstane than have let it run them into sorrow o' that kind.

Angus.—Man, surely your wark has na been gaun weel wi' ye the day, that ye're talking that way.

Howie.—Its spoken in reason though.

Angus.—Ay, wi' reason o' the kind that would have left us still feeding our horse-power wi' beans. The back-ground o' effort that never gets to the front is to be pitied nae doubt; but there's poetry in't man, if man had but an ear for't, and the Lord kens the world's fou o't. An empty meal-poke, and

an abstracted mind, that's aye awa trying to see things before their time, gang weel thegither; but the grundstane Mack speaks o' would rub poetry out of the companionship and leave naething but sair places.

Howie.—But, Angus, the background cannot be aware of this compensation to their disappointments.

Angus.—Oh, I never was that much in the background tae ken whether they are or no. I'm no in the way of sair hearts. If folks dinna play fair I soon let them hear o't. But we did na come to speak aboot that. Howie wants to speak to ye aboot hoo to get up the way the birds dae.

Howie.—I've been simply wishing to know how birds fly.

Mack.—I fear you are not alone in your wish. I'm somewhat at a loss mysell: there's sae many different kinds of fleein.

Angus.—Oh, Howie's no particular, he's willing to employ ony o' their ways.

Howie.—I've been reading Marey on the motions of the wing, but wish to know how support from the air is derived from these motions.

Angus.—He wants to ken, in fact, in what kind o' fashion the stour would rise, supposing the air road were a dusty ane.

Mack.—I doubt stour's no the proper word, for if the air that has been compressed by the forepart o' the wing reacts on the flexible rear edge, as there's some reason to believe it does, it should not have much motion left in't tae raise onything like stour when the wing has passed.

Howie.—My difficulty is this. I am at a loss to know how the force of gravity in a falling weight is expended on the air when wings are used. From the wings in motion we get a certain pressure, but how does this pressure stand in relation to the force of gravity?

Mack.—Weel, the pressure is the resistance tae displace-

ment o' the weight o' the air come upon. The force o' inertia o' the weight o' air displaced balances the force o' an equal weight o' the displacing body, sae that when, at a given speed, the force o' inertia in the air displaced becomes equal tae that o' the whole weight o' body, the displacing speed is found tae continue uniform, because the inertia forces are balanced.

HOWIE.—Yes; but how can we, from the elastic pressure, determine for weight of air displaced? for the pressure is constant, and it is not clear how constant pressure can be an equivalent to the displacement of a weight of air whose volume must take time to get out of the way.

MACK.—The resistance is as constant as the pressure. The imposed pressure performs work in the displacement o' air, and has in itsell tae be constantly renewed by fresh force, even as the air equivalent is renewed by fresh air come upon.

HOWIE.—Yes; but supposing the weight of a bird to be 1lb., will that require pressure amounting to 1lb. in the wings?

MACK.—If it did the bird could not fly. The bird has only, in a given time, say 1 second, tae develop in its wings dynamic force equal tae the dynamic force that gravity would develop in the 1lb. weight in that time; but tae save further explanation at present I will give ye some notes bearing on the question, that ye may examine them at your leisure. Ye'll see they're already in print.

ANGUS.—Oh, Howie 'll no understand them. Talk them tae him, man, and leave oot the figures.

HOWIE.—We'll set the figures to music, for Angus's bag-pipes.

ANGUS.—And we'll tak you for the win bag: ye'll be o' some service among figures then.

HOWIE.—Whose wind will be in the bag in that case, Angus?

ANGUS.—Gang on wi' your crack; time's pressing, ye ken.

Howie.—Well, a body falling freely in space, and the same body falling in air, would be under very different conditions as regards velocity. The velocity of the body falling in air is retarded, and at length becomes uniform with the space fallen in a given time; but it is not so with the body falling free from air. Now, in reckoning the actual energy, or force accumulated in the weight in a given time, by taking the actual fall in air, and for the same space of time in a vacuum, I cannot bring the two cases to common terms.

Angus.—Twa's ower many for ye to manage at ance, Howie. Tak them singly and gang lightly.

Mack.—I see your difficulty; but the energy is in the final velocity, irrespective of the distance fallen to acquire it; and in the case of air, as the velocity at any point determines the actual energy of the pressure on the air, we refer it only relatively as to a standard rule, to a similar velocity in a vacuum or free space, the distance fallen in the vacuum determining the velocity. For convenience of observation of the uniform acceleration of gravity, it is usual to have two unequal weights hanging from the two ends of a line which passes over a pulley delicately balanced. The gravity of the slight excess of weight on one side forms the motive power that sets the two weights in motion, the lighter upward and the heavier earthward; and the motive power is so small compared to the inertia resistance of the whole weights it has to put in motion, that the velocity, though uniformly accelerated as in a free fall, increases with slowness that bears a distinct relation to the ratio of the excess weight to the whole.

Angus.—But what does the excess weight correspond to in a case o' fleein?

Mack.—It must be looked for in the weight of the fleein body, only before it has got complete air support, and has, therefore, an unbalanced downward tendency. If in the room of

the lighter restraining weight we substitute air resistance beneath the motive-power weight, and make that weight a wing plane, we are free to reason about the inertia of this air resistance as we would about that of the weight it took the place of.

ANGUS.—Mack disna ken ye as I do, Howie, but I'll straighten it all out for ye on the road hame. We can jist noo, at least, look wise and say naething. I've known a cheap advantage got that way whiles. The talking folks begin then to tak care what they say, out o 'respect like. But, Mack, that's only between Howie and mysell, ye ken. We're baith waiting on ye.

MACK.—In the case of the wing-plane pressing upon air ye may not, in relation to time, be able to bring the retarded velocity to common terms with the free velocity of gravity, but ye can the spaces fallen up to the moment when the velocity of the wing-plane becomes nearly uniform; and the tabulated results of one of M. Didion's experiments, given in Bennett's Morin, shows this. If, on the shorter of the two legs of an L figure, ye mark off the timed spaces actually fallen, and on the longer the spaces due in natural gravitation in the same times, ye will find that lines connecting these space points will run parallel from the start at zero to the point he reached near uniform velocity of plane, at the end of 2 seconds of time.

ANGUS.—Ye say the time of the experiment was 2 seconds. Weel, if dynamic force rules in the case, will the wing-plane falling 2 seconds in air hae developed force equal to the force it would develop in the same time falling in a vacuum?

MACK.—Well, in this experiment the final velocity was only about one-third the velocity due to free gravity, and we can compute by the velocity only.

ANGUS.—At that rate it seems to me that the air force could be only about a ninth o' the free space force, and, at the

most, ye hae only the weight and its equivalent air pressure, equal 2. Noo, what has become o' the difference?

MACK.—At the end of the first second the acquired velocity was about one-half that due to free gravity, the space fallen being in about the same proportion. Close to the start we find the space fallen in the higher ratio of about six-tenths of the space for free gravity, and the acquired velocity would no doubt correspond, though it is not given. We here then see the difference beginning at the start, where the velocity is small and the force consequently feeble; and to bring the difference to a balance with the force developed in free gravity we would have to estimate the whole work done in the respective cases in the given time.

ANGUS.—Weel, we'll no bother wi' the estimate, we'll tak your word for't. If ye could give us an estimate o' the difference wi' a man looking out o't ye might depend on our keeping mind o' what ye said.

MACK.—No doubt I would if I could, Angus, but I hae na finished wi' what I was saying. The small velocity at the start shows, very sensibly, that the actual full weight of the wing-plane is not borne by the resisting air when the weight first begins to move, for the weight requires time to get up its speed, and the resistance of the air is according to the speed of its displacement. The attraction of the earth has had the same time in both the cases that concern the difference, and is a uniform force, irrespective of whether the body has motion in it or is at rest; and as there is only the air resisting, the air must have had the force transferred to it that otherwise would have accumulated in the weight; but we have already had some talk on that matter.

ANGUS.—We have, and dinna begin again. I think, Howie, we maun gang. I have tae catch the night's post, and I doubt I'll have tae rin.

HOWIE.—There are many other points I would like to speak of, but we may have another opportunity.

ANGUS.—He means to say, Mack, he feels nae nearer fleein when he's gaun awa than when he cam. Your philosophy has na been pictorial enough. He would raither hae found ye up tae the knees among shavings, wi' wings at least ready for the glueing; and I'm no sure but a glue-pot would hae made things livelier. I'm kind o' disappointed mysell.

III—EXPERIMENTAL BELIEF.

ANGUS.—Weel, Mack, hoo are ye the day. A bonnie afternoon is'nt it? I've jist been givin the weans here a walk on the hill-side. Man, but it's pleasant wi' the sun shining and the wind blawing wi' summer saftness. The heart feels that glad and cheery that grey hairs gang for naething in the thoughts. I was wishing ye had been wi' us; but, maybe, a man needs tae be a faither before he can enjoy himsell among bairns as I can, and mine are nane o' the quietest. I'm thinking some o' them'll need some room tae work in when their beards are grown. I'm doubting they'll be asserting the rights o' man before the rights o' their faither hae quite dune wi' them.

MACK.—Man, I envy ye.

ANGUS.—Ay, in a contemplative kind o' way; but they've a' had the measles and the hooping cough, and there's naething wrang noo but broken windows, and arms and legs growing faster than their clothes. Ye've only tae look at Tommy's face tae ken wha broke the windows.

MACK.—Eh, Tommy, man, but ye're beginning your sorrows early. The tailor maun gie ye pouches tae keep your hands in, and ye'll no break windows then, ye ken. But here, my wee man, here's a ball I got for ye yesterday; ye'll no break ony wi' that ane, and there's a whustle in the air-hole. But what's that ye hae got wi' ye, Andrew? Let me see't? Is't a new kind o' windmill, or what is't?

ANDREW.—It's an ariel.

MACK.—This is no o' your ain devising I can see, Andrew. A thin card-board hoop, centred wi' thread on a light rod near one end, wi' feathers stuck sloping oot frae the hoop edge, Angus, did ye manage in your walk tae get this up? I'm sorry noo I was na with ye.

ANGUS.—Ay, I canna say we did na; but there's something no yet thought oot properly.

MACK.—That's no tae be wondered at; but had it ever far tae fall?

ANGUS.—Weel, I intend to mak the next ane lighter. I think that's what's needed before we begin tae speak aboot results.

MACK.—Ay, ay: we're no needing ony mair information aboot the force o' gravity. We want sustenance noo. But which end gangs first: the plain edge o' the hoop or the feathered edge?

ANGUS.—The plain edge; and ye'll see that the feathers are set tae act screw-propeller fashion, at least they were when we left hame; but, man, it was only to please the bairns.

MACK.—Ay but, Angus, I'm much mistaken if ye were na thinking a while by yoursell, withoot the bairns being in your mind. The feathers here tak rank wi' the outer ends o' propeller blades, the length o' the feather answering for the breadth o' the blade: but, man, they're far ower long, and ye hae set the wrang face o' them tae the pressure. Ye see the

rib o' this feather's no in the middle. Ye should hae had the broader side o' the membrane outwards, for the narrow side, being stiffer, should come first on the air; but I see ye hae set some right and some wrang. Why your feathers are a' rights and lefts, and some o' them are tail anes. Man, the air would hardly ken which way ye wanted the thing tae gang.

ANGUS.—Weel, ye see, it was'na a fleein bird they were got frae, and the theory needna fa' out wi' the feathers, for it disna appear tae be a fleein ane either.

MACK.—Ay, but look ye, if the feathers had been o' the right sort, and o' a third o' the length ye hae them, wi' the flexible side o' the membrane inclined outwards, ye would hae got some pressure outwards upon the surrounding air, getting new air at an angle laterally as the thing advanced, and that would hae given it baith sustenance and steadiness on its path. If the tip o' a bird's wing did na bend so as to direct some pressure laterally, it would lack steadiness in a straight course and power in turning. By keeping the feathers here short in the direction o' their motion, which would correspond tae a narrow propeller blade, the air that is come upon finds that the end o' the feather has passed and got a' it needs before the pressure has had time tae free itsell by lateral diffusion, and as this diffusion would cause motion in the surrounding air, the long draggle ends ye hae been using would, as they cam up, find the firmness o' the road sae much lessened as tae be nae road at a' tae speak o'.

ANGUS.—Oh, they did na lang want for support when I gied them their liberty, and there was nae diffusion worth considering; but if ye like I'll mak ye a present o' the whole apparatus, motive power bobbin and a', tae work your improvements on.

MACK.—Man, but I hae nae weans tae be an excuse like when trying 't. Folks would think I was in earnest gaun

alane. Besides it is not the form I would adopt were I in the way o' experimenting.

ANGUS.—Let's hae your mind on the matter and ye shall hae baith the weans and mysell at your service. Weel hae a fleeing machine this time surely, Tommy.

TOMMY.—And will't gang ower the trees?

MACK.—Would ye like tae see't gaun ower the trees, Tommy?

TOMMY,—Yes, this ane did na; but faither said he would mak a big ane some day and tak me in't.

MACK.—But the birds would laugh at ye, and ye might fa' aff, ye ken. Ay, ye may look at your faither. There's naebody but birds gang ower the trees.

TOMMY.—But faither said he would tak me.

MACK.—I doubt, Angus, ye'll hae tae keep your word here. When the bairn has faith in his faither sae far as that there's nae help for't, but flee ye maun tae save his faith, for, man, it's precious. Ye're weel aff to hae somebody tae believe in ye.

ANGUS.—Ye maun help me a' ye can then. Ye put the case in that light would mak me risk mair than birds laughing; and ye ken a body'll no can keep the thing in a corner till a's ready. What kind o' form is't ye were saying ye would adopt?

MACK.—Oh, I was only speaking frae the easy side o' the question. That's the side the maist o' folk are speaking frae noo, and I canna say that I've ony positive idea o' the thing that's wanted.

ANGUS.—Man, what think ye o' the wave o' expansion we've had the talk aboot? On the rear edge o' the wing ye'll mind. We would jist need some lithe frame-work, and a dozen yards or sae o' holland, tae fit us up.

MACK.—Ye're joking surely?

ANGUS.—Faith, I'm no sure if I am. I was watching some what ye call soaring birds, big anes, the ither day in the

course o' my travels. They were aye ganging, and there could be nae magic in their performances, naething but their twa wings, and natural aptitude in the way they held them. Man, I never saw sic easy work sae simply dune.

MACK.—But, Angus, the simplicity ye speak o' is proving mair difficult tae comprehend than the laborious style o' rowing birds. Ye had better try some ither example where the mechanical forces are mair apparent. It would be hard tae find a mechanical equivalent tae the organic sensibility operating through the shouther joint o' the soaring bird.

ANGUS.—Mechanical forces and laborious style; that means motive power and weight o' engine essentials. Tommy, wi' a' that tae carry I doubt we'll no can baith gang thegither, we'll be ower heavy.

MACK.—Hout man, dinna talk that way; ye can jist gang wi' the fewer coals.

ANGUS.—The natural aptitude can gang withoot ony coals at a'; and gang sae simply, that I was only surprised that nae ither body's aptitude than the birds' had yet been put in use. But, Tommy, my man, never mind. If your faither disna see the way tae tak ye, ye'll maybe some day tak your faither.

MACK SOLILOQUISING

WHEN ANGUS HAS LEFT HIM AGAIN ALONE.

A trouble of the mind, this lingering thought of flying; a central weakness in it, like a child in a tramping company. A vagary, ill at ease in presence of the judgment, that knoweth not how to fit it in, and cannot well allow it to stay there, and yet cannot bid it go, for the breadwinning forces seem but dull fellows after all when looking from them to it. An ancient dream,

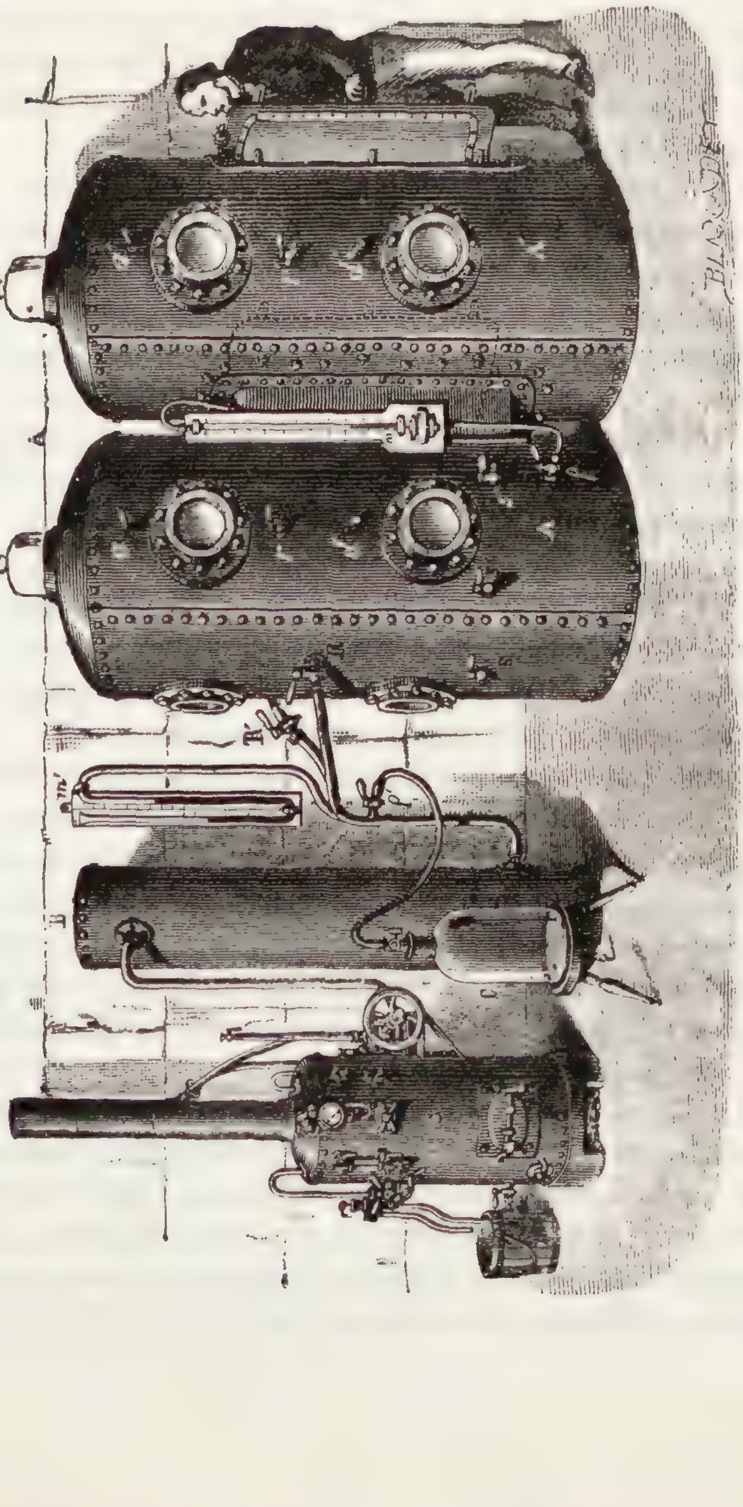
seeking anew to find a voice among the thoughts to give it utterance; but the minds it fain would interest will not dream, or are unwilling to own that it hath seen encouragement to visit them.

Over the watery waste and along the ground, a weary way to where many of our best people, widely wandered from our midst, are outlying, and now and again in places becoming lost to knowledge, so that we know not whether they be dead or living. An open highway to them in the air, but no one on it. Faith rules in other matters, and according to faith must it be here, where the eye sees not the road that has to be travelled, and where the action of belief is needed to develop its reality.

M. Paul Bert, President of the Société Française Aërienne, is the author of Experimental Researches upon the influence exercised by changes of barometric pressure upon the Phenomena of Life, and is this year the recipient of the grand prize biennial of the Institute of France. The work itself contains results of the highest importance to aërial navigation.

The following extract is taken from the November Bulletin of the Society's proceedings, by James Glaisher, F.R.S.:—

"The results obtained by M. Bert bring to light this remarkable fact, that, according to the proportion employed in respiration, oxygen becomes either an aid to life or a poison.



"The apparatus used by M. Bert for these researches was placed at his disposal by Dr. Jourdanet. One apparatus employed consisted of two closed cylinders of thick iron, communicating with an air-pump set in motion by a Lenoir movement.

"In one of these cylinders Crocé-Spinelli, Sivel, and M. Bert himself tested, in their own persons, the effects of a rapid diminution of atmospheric pressure. M. Bert, by the respiration of oxygen, submitted therein to a pressure of 9·44in. of mercury, which, fatal as it would have been without this precaution, caused him not the slightest inconvenience.

"Respiration, it has thus proved, might be maintained at a height of about five miles by absorbing 610 cubic inehes of oxygen per minute.

"The first ascent performed by Crocé-Spinelli and Sivel, in 1874, attending to the instructions given by M. Bert, succeeded perfectly and gave important scientific results, the two aëronauts describing, on their return, the great advantage they had derived from the inhalation of the oxygen.

"It unfortunately happened that when our colleagues made their ascent on the 15th April, M. Bert was at Auxerre. Sivel was unwilling to wait for his return. The two adventurous friends, trusting to their own intrepidity, carried with them an allowance only of 140 litres for each, that is to say, sufficient for 14 minutes only. M. Gaston Tissandier, who had never taken part in a high ascent, suffered himself to be guided by his colleagues.

"The aëronauts, wishing to economise the oxygen, reserved it for the greater heights of their ascent, and did not begin to inhale it soon enough. Now, the ill effects experienced in balloon ascents is insidious, and when the explorers wished to have recourse to these means of safety, they found themselves unable to raise the tube to their lips.

"The death of our friends is in itself therefore an example of M. Bert's theory, and shows that if we hope to continue the series of extreme high ascents, it will be necessary to furnish aëronauts with automatic apparatus for the inhalation of oxygen.

"The institute has fully comprehended the importance of M. Bert's work upon the influence of barometric pressure.

"M. Lefuel, presiding at the Annual General *Séance* of the Five Academies, expressed himself in the following terms :—

"'A biennial prize of twenty thousand francs is, by turns, accorded to that work or discovery most calculated to reflect honour upon, or be of use to, the country.'

"By the terms of the decree of the 12th of December, 1860, this reward, the most honorable that could be to national emulation, is decreed by the institute upon the successive nomination of each of its classes. This year, upon the proposition of the Academy of Sciences, it has been granted to M. Bert, Professor at the Sorbonne, for his work upon the influence of barometric pressure upon the phenomena of life.

"In granting such a recompense to the numerous and varied experiments of M. Bert, to his useful and long-continued studies pursued for many years under very difficult circumstances, you have, Gentlemen, to speak in the name of the Academy of Sciences, represented by M. Claude Bernard, you have, I repeat, made clear to every one the importance you attach to the progress of pure science and to the discoveries of scientific truths. These last are always fruitful, but time is required to develop and mature results. The discoveries of M. Paul Bert possess this eminently scientific character of certainty and precision, which at once places them in the front rank of the greatest physiological discoveries of our epoch.

"Not only is the Society of Navigation Aërienne honoured in the person of its President, but it derives satisfaction from the fact that the biennial prize has been decreed for a subject bearing upon aërostation.

"The time has long since passed when thought and enterprise spent in the service of aërial navigation were looked upon as folly. It is certain, however, that the study of aëronautics is not exempt from followers who, speculating upon the credulity of the public, endeavour, as we have seen lately, to abuse its confidence by illusive prophecies: but if amongst those engaged upon aërostation there should be charlatans, we should, under no pretext whatever, admit them as colleagues. There are others, it may be seen, that France delights to honour, like our President, M. Bert, or that France deplores, like our colleagues, Crocé-Spinelli and Sivel.

"FELIX CARON."

"*Monthly Notice for November,* 1875, *of the Société de la Navigation Aërienne.*"

EXPERIMENTAL RESEARCHES,

BY

Prof. M. Paul Bert, *Deputy of the National Assembly,*

UPON THE INFLUENCE EXERCISED BY CHANGES OF ATMOSPHERIC PRESSURE UPON THE PHENOMENA OF LIFE.

Translated by JAMES GLAISHER, F.R.S.

The various notices I have had the honour of presenting under this title have had the effect of demonstrating that changes of barometric pressure, if we except very rapid and great decompressions, have no physico-mechanical action upon animals and vegetables, but influence them exclusively from a chemical point of view. Below the normal pressure of the air too feeble tension of oxygen tends to promote asphyxia: above, too strong a tension tends to increase those formidable accidents which I have designated, somewhat paradoxically I admit, by the expression, poisoning by oxygen; and hence the conclusion at which I have arrived, that all danger may be avoided by varying the oxygenous richness of the air inversely to the variation of pressure. Thus, as regards the diminution of pressure, the *mal des montagnes,* and the *mal des aërostats,* I have said—

"If aëronauts, stopped in their vertical career not by the failure of the balloon's ascensional force but the impossibility of maintaining life, should wish to ascend to a greater height than has yet been done, they will be able to accomplish their

desire by carrying with them a small balloon filled with oxygen, to which they will have recourse when suffering from the rarefaction of the air."

On the 20th of last March, at 2h. 37m., I placed myself in my great apparatus of decompression, within which the temperature was 53·6°, and the atmospheric pressure 29·69in. Under the influence of the pumps, which maintained a current of air with a constantly increasing pressure, at 3h. 10m. I found myself at 17·72in., and maintained myself until 4h. 20m. between that pressure and that of 16·06in., values corresponding to heights of 13,431ft. and 15,712ft. respectively. I then reascended to the normal pressure, which I attained at 4h. 45m.

On arriving at 17·7in. I began to experience symptoms of the *mal des montagnes*. These continued to increase up to the moment of the decompression, and consisted of a feeling of heaviness and weakness, with sickness, fatigue of sight, general indifference, and inertness of mind difficult to surmount. On attaining a pressure corresponding to the level of Mount Blanc, it seemed to me impossible, after counting my pulsations during the third of a minute, to multiply by three the number found. A little later, having lifted my right leg, it was seized with convulsive tremblings, which extended to my left leg and lasted some few minutes. My face was then slightly congested, and the temperature underneath my tongue, taken with the greatest care, presented an increase of 0·1 to 0·2 of a degree. My maximum respiratory capacity, measured by the spirometer, had lessened in the relation of 17 to 12. Lastly, under 17·7in. of barometric pressure, I found it absolutely impossible to whistle.

These facts, however, I do not here insist upon. The interesting point of my experiment is as follows :—

I had taken with me a little balloon of nearly pure oxygen. On arriving at nearly 16·9in., with very manifest distress and

a pulse which, from 62 pulsations, had gradually increased to 84, I made an inspiration of oxygen. Almost immediately my pulse fell to 71. It soon reascended, the more so that I made an effort to breathe into the spirometer, and reached 100 only to redescend spontaneously to 90. The same experiment was repeated ten times during my stay, and each time the same result was produced.

Each respiration of oxygen was accompanied by a very disagreeable *eblouissement.* Having on one occasion made three consecutive inspirations, I very nearly fell from my chair, seized with vertigo; but this effect soon passed off and was followed by a short period during which all sense of sickness disappeared and my pulse reascended. The violent sensation immediately following the inspiration of oxygen is easily explained; in fact my oxygen, under a pressure of 16·9in., had a tension corresponding to that of oxygen contained in the compressed air of 2·5 atmospheres. I therefore passed suddenly, as regards chemical tension, from nearly 1·5 atmosphere to 2·5 atmospheres, a shock which could not fail to be attended with some inconvenient effect; but it remains none the less established that all sickness (the *mal des montagnes*) disappeared, and that the circulation returned to its normal rhythm under the influence of one single inspiration of oxygen.

MM. Crocé-Spinelli and Sivel, desirous of preparing themselves for their high ascent of the 22nd March, experienced analagous effects. I subjected them to a pressure of 11·8in. M. Sivel, who was possessed of an excellent physique, was not affected below 15¾in. M. Crocé, of less robust constitution, was very speedily attacked. At 11·8in. his lips were blue and his ears nearly black: he was asphyxiated. Now, one inspiration of oxygen alone caused in a moment all these formidable symptoms to disappear. The pulse fell; respiration became free. At the moment when M. Crocé became blind

oxygen suddenly restored him his sight. But they had, like myself, experienced the impossibility of regularly breathing pure oxygen. I therefore gave them to carry on their voyage two mixtures of air and oxygen, the one contained 45 to 100 of carburetted gas, the other, 75 to 100, was reserved for the greatest heights.

I will leave to the two intrepid aëronauts the honour of exhibiting the important results of their successful ascent. I will only add that without oxygen they would probably have been unable to attain regions where they found again, with a temperature of minus 7·6°, the 11·8in. of pressure which they had supported in my apparatus. Without oxygen M. Sivel could not have lifted the bags of ballast, nor M. Crocé-Spinelli have seen the lines of the spectrum he went on purpose to observe. They breathed the mixtures without experiencing the *eblouissement.*

I was desirous of testing upon myself the effect of the continuous respiration of a sur-oxygenous mixture. In a first experiment I was able, by employing a mixture of 45 to 100, to lower with impunity the pressure to 13·3in., corresponding to 17,373ft., the height of Chimborazo. In a second, with a mixture of 63 to 100, I descended to 9·84in., and I should have gone lower still if my machine had been more powerful. I only began to breathe oxygen after experiencing some inconvenience, and at the moment when my pulse had very considerably augmented. From this moment all disagreeable symptoms disappeared.

A sparrow I had placed beside me all but perished; its temperature having decreased from 41·9° to 36·1°. The pressure to which I attained without sickness, thanks to the oxygen, was that at which Glaisher and Coxwell fell insensible at the bottom of the car. It corresponds to the height of the most elevated of mountain peaks, the Gaourichnika,

which is henceforth theoretically accessible. I think it possible in this manner to attain the pressure of 5·9in. Mr. Glaisher was therefore right in saying—"I have no doubt that observations will ultimately be made in regions to which I have been unable to attain without loss of consciousness. It is not for me to take upon myself to determine the limit of human activity."

The following Paper, by M. A. Gaudin, was communicated to the Société Française de Navigation Aërienne shortly after the scientific ascent which preceded the late fatal balloon accident:—

Upon the employment of Oxygen mixed with Atmospheric Air in Respiration,

BY

M. A. GAUDIN.

Translated by JAMES GLAISHER, F.R.S.

Apropos of the very remarkable effects of the respiration of atmospheric air enriched with oxygen confirmed by MM. Crocé-Spinelli and Sivel during their last aërostatic ascent, I remember to have obtained some long time since very analagous results.

This was in the year 1832, on the occasion of the great epidemic of cholera. A young physician employed me to administer to the cholera-patients of the ambulance of the Rue Grange-Batelière pure oxygen to assist in producing re-action. We operated upon the sick in the last stage of the malady, and some were saved by the employment of this means.

M. Touzet immediately conceived the idea of creating an establishment for breathing air enriched with oxygen, as a preservative against cholera, and he confided to me its direction.

In the meantime the cholera disappeared, and only a few solitary attempts were made by the aid of the apparatus I had mounted.

M. Touzet prepared a mixture, consisting of equal parts of atmospheric air, oxygen, and extract of the per-oxide of manganese, and caused some persons to inhale it, upon whom it had the same effect as champagne.

For my own part, I tried the experiment, at various times, upon myself, by the aid of a suitably-arranged tube, and each time I obtained an analagous result, that is to say, an extraordinary sense of freshness and relief which took from me all desire to breathe again, so that on closing my mouth and holding my nose I could remain for more than five minutes without experiencing the least sensation of suffocation.

Nothing could be easier than to repeat this experiment, in order to ascertain its entire bearing. It might furnish a very important application for the service of divers employed in the inspection and recovery of sunken vessels, and more especially for fishers of sponges, corals, and pearls, if, by the aid of so simple a means, we could triple and quadruple the duration of time that a diver is able to remain under water.

CONCLUDING REMARKS.

The preceding Papers contain information which would otherwise have found a place in Concluding Remarks: these will therefore be very brief.

The attention of Members is called to a Prize offered by the Tayler Society at Harlem, "for a critical explanation of what observation and theory have taught us concerning flying, followed by the author's researches, experimental and theoretical."

The Prize will consist of a Gold Medal, struck by the Society, of the value of 400 florins.

The Papers must be written by another hand than that of the author, and forwarded before the 1st of April, 1877, after which date no further addition will be admitted.

The Prize for the best Essay will be declared before the 1st May following.

All the Papers sent in will remain the property of the Society, who will insert in its publications, either with or without translation, the accepted Paper, the author renouncing all right of publications except with the authority of the Society. It also reserves the right to make such use as it may think desirable of the unsuccessful Papers, either with or without mentioning the name of the author; but if the former, previous consent will be sought. All Papers to be signed only by a simple device, to be repeated in a sealed letter attached to the Paper, which letter must also contain the name and address of the author, the whole to be forwarded "Á la Maison de la Fondation de feu, N. P. Tayler, Van der Hulst à Harlem."

An attempt was made by Mr. Simmons, the aëronaut, to supplement the employment of a balloon in warfare by the employment of a kite, which, from the peculiar nature of its construction, he designated the parakite. It was, in fact, a combination of the parachute and kite. It looked like an enormous umbrella of loose cloth before inflation. When distended by the wind, the upper wires—extending from the central pole to the end of the diagonal bamboos—retained the cloth in its place. On each side of those two diagonals, which were at right angles to the direction of the wind, the cloth was intentionally left in two loose folds, so as to leave an opening for the compressed air to escape, on the principle of the Japanese kite.

The theory was that the parakite would descend upon two columns of air. This was so in practice, but there was a difficulty in effecting its ascent. It was intended upon the last occasion at Chatham, when under trial by the Royal Engineers, to send up one of much smaller dimensions, and then to attach the larger one, but the wind being boisterous and gusty the larger one was fractured before ascent, so that the smaller kite alone was experimented upon. The poles which extended diagonally the square surface, were each 14ft. 6in. in length. It rose to the height of about 200ft. To that height it rose steadily with 60lbs. of ballast, being double its own weight, but refused to mount higher. On attaching another 60lbs. it rose wildly and then swerved violently downwards, knocking down a couple of boys and smashing itself. Repairs, however, are easily effected with the aid of spare poles which run into sockets.

The larger parakite extended about 700 square feet of light waterproof material. The diagonal bamboos were each 30ft. long, and the weight of all was 86lbs.

It is to be regretted that on the several days appointed for

the experiments no trial could be made for want of wind, except upon the last occasion, when there was a superabundance. Such a machine requires a number of trained men to manipulate it effectively, and just as they were becoming acquainted with the required action the experiments ceased.

Mr. Simmons has since that time effected improvements in it.

The Californian flying machine again crops up during the past year. The sustaining power of gas, which was depended upon to help in part the action of the extended planes when propelled against the air, has been quite abandoned, and the machine now consists wholly of plane surface, steam engine, and screw propellers. Instead of the planes being extended laterally, however (for there are three), they are superposed longitudinally, with an interval of about 10ft. In length the whole structure is 120ft., fixed upon a foundation of trussed bamboo. The planes are unequal in length, the largest on the top being of the above dimensions and about 40ft. wide.

These three planes are rigidly supported by two masts about 40ft. high, and stayed by wire rigging.

To the lower end of each mast is affixed a small wheel, to run down an inclined single rail, so as to impart the necessary initial velocity. This is then to be continued by means of an engine enclosed in a square compartment, capable of holding the engineers. This compartment is fixed in the centre of the trussed bamboo keel. The engine works four screw-propellers, two vertical and two horizontal. Their place of working breaks the continuity of the longitudinal planes.

The weight of the whole machine is calculated to be 1,500lbs., this is inclusive of man and motive power, &c.

It was to have been tried in November last, but we have not received further particulars.

At the General Meeting of the Société Française de Navigation Aërienne, held on the 3rd December, 1875—President, M. Paul Bert, Professor a la Faculté des Sciences—M. le Docteur de Villeneuve, the Secretary General made the following observations :—

"Our Society is not the only one which occupies itself in the study of Aërial Navigation. The Aëronautical Society of Great Britain has been founded these ten years by Mr. Fred. W. Brearey, who performs the duties of Honorary Secretary, a position corresponding to that of Secretary General with us."

After passing some complimentary observations upon Mr. Brearey's exertions in the cause, he said—

"The French Society thought it only justice to show its appreciation of his services by awarding him its Gold Medal. Mr. Willoughby, the English Vice-Consul, acknowledged the compliment paid to his countryman, and undertook to hand it over to Lord Lyons for transmission to the Duke of Argyll, the President of the English Society, who, at his Grace's residence, delivered the same to Mr. Brearey."

In the completion of this, the Tenth Annual Report, we may look back with some amount of satisfaction at the gradual retirement of some of the imaginary obstacles which puzzled and bewildered the earnest inquirer into the principles of flight. The readers of our Reports cannot but be impressed with this truth. In applying the knowledge thus attained to the accomplishment of flight, mechanical difficulties have yet to be surmounted. Expensive failures are aiding in this object.

It cannot now be said that the want of a light motive power presents any difficulty.

It has often been asked if the Society offers any prize for the successful achievement of flight by man? The answer ought to be obvious—that no amount of money which could

be offered by the Society would adequately reward success. The remuneration must be looked for, and would doubtless be realized, through other sources.

It might, however, be a question whether the Society should, by the aid of its Members, offer prizes for models which shall be capable of imitating the flight of selected specimens in nature, such for instance as the stag-beetle, the butterfly, the dragon fly, the hovering of the hawk, or the flight of the swallow.

Some very effective models have been constructed by MM. De Villeneuve and Penaud of the French Society. They are very light and somewhat evanescent in the duration of flight, but certain conditions, as to weight-carrying capacity, might be attached independently of size, and they should be capable of flying a certain distance independently of time. It must be observed that no open air demonstration is feasible, as an apparatus of some weight can alone contend with the ground currents. A large space like the Central Hall at the Alexandra Palace would suffice for every condition. We leave this suggestion to fructify with such of our Members who may approve of the suggestion, and can aid in contributing to a handsome prize for so interesting an exhibition.

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THE FOLLOWING

SPECIFICATIONS OF PATENTS.

No.	*Date.* 1875.	*Subject.*	*Patentee.*
140	Jan. 14.	Improved Apparatus for Navigating the Atmosphere... ...	John O'Connell Cave.
169	Jan. 15.	An Improved Kite or Aërial Apparatus for Military and other purposes (Communicated by F. C. U. P. d'Esterno) ...	A. M. Clark.
289	Jan. 23.	Improvements in purifying gas, the means and Apparatus for inflating and rendering ascensive Balloons and other Aërial Machines, and in the Apparatus employed therein	J. Simmons. J. M. Morris.
594	Feb. 17.	Improvements in the Means and Apparatus for generating fluid to work Engines so as to develope great power in proportion to bulk and weight, more particularly applicable to Aërial Locomotion	M. P. W. Boulton.
1690	May 6.	Apparatus or Means for Propelling and Steering Balloons	J. S. A. Menier.
2428	July 5.	Improved Means and Apparatus for conveying or carrying human beings or objects into mid-air	J. Simmons.
2901	Aug. 17.	Improved Method of, and Appatus for, Steering Balloons ...	D. Biddle.
2979	Aug. 25.	Balloons	H. Mc Kee.
3315	Sep. 22.	Improvements in the Navigation of the Air and in Apparatus therefore (communicated by E. Vidat)	P. Jenson.
3369	Sep. 22.	An improved Aërial Vessel for Maritime and Fluvial Navigation (communicated by B. Picard and A. Lawrent) ...	A. C. Henderson.
4151	Dec. 2.	A new Flying Machine	J. K. Smythies.

BOOKS, PAMPHLETS, &c., RECEIVED.

Memoir sur la Navigation Aérienne, par M. Ménier.—By the AUTHOR.

Smithsonian Reports for 1873-4.—Presented by the Smithsonian Institution, Washington.

Rivista Degli Studî di Locomozione e Nautica nell' aria Di P Cordenons, Prof. di Matematica nel R. Liceo di Rovigo.—By the AUTHOR.

Aviation—Appareils de Vol Méchanique, par M. A. Pénaud—By the AUTHOR.

Navigation Aérienne Sérieuse, par Vaussin Chardanne Ingénieur Civil—By the AUTHOR.

L'Aéronaute, Monthly Reports of the "*Société Française de Navigation Aérienne.*"

Projet d'un Aérostat propre a la Navigation Aérienne suivi d'un projet d'Aérostat—Observatoire pour le service des Armées en Campagne, par C. Fiess—By the AUTHOR.

Eleventh Annual Report

OF THE

AËRONAUTICAL SOCIETY

OF

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FOR THE YEAR 1876.

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Eleventh Annual Report

OF THE

AËRONAUTICAL SOCIETY OF GREAT BRITAIN,

FOR THE YEAR 1876.

Containing an Account of the Proceedings and a Selection from the Papers and Communications received by the Society during the year, with Concluding Remarks upon the present state of the Science.

THE ANNUAL MEETING of Members of this Society was held in the Rooms of the Society of Arts, Adelphi, on Wednesday Evening, the 7th June, 1876, at Eight o'clock, for the reading and discussion of Papers and the general advancement of the Society's interests. Mr. CHARLES BROOKE, F.R.S., was called upon to take the Chair.

The Minutes, having been previously approved at a Meeting of the Council, were taken as read.

The CHAIRMAN called on Mr. D. S. Brown, who read his Paper on

THE ADVANTAGES OF EMPLOYING POWER FOR AËRIAL PROPULSION IN AN INTERMITTENT MANNER, AND ON THE SOARING OF BIRDS.

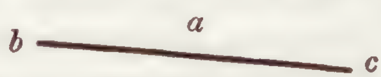
I have before directed attention to the advantages of employing power in an intermittent manner, and at the General

Y

Meeting of the Society in May, 1874, I gave a practical illustration of it. I have now to submit a modification of the principle, which I believe to be well adapted for aërial propulsion. Suppose that the rods or poles connecting the planes of the bi-plane, which I then described, to be substituted for a tube, the tube also to serve as the cylinder of an engine, to be worked by the explosion, at short intervals, of petroleum gas, giving motion to a piston having a parachute propeller fixed to the outer end of the piston-rod, so as to act directly on the air by expanding as it is driven one way and collapsing when drawn the other. By this means it is probable a sufficient velocity can be attained, which I fear will be difficult if not impossible to accomplish by any rotary propeller.

I have also lately devoted some attention to the soaring of birds, with a view as to the direction in which our first experiments in flight on a large scale should be made, and for which the bi-plane, on account of its great stability and the facility which it affords for balancing, is well adapted. I should state that when the planes are rigid and not inclined, and the centre of gravity is midway between them, it will neither pitch nor skim, but assume a horizontal position when not propelled, and so fall gently to the ground. But if the weight be at the extremities or moved somewhat forward, and the bi-plane be at the same time inclined, it will then glide downwards with great rapidity by the force of gravity alone, on account of the oblique manner in which its under surface encounters the air. If, again, it be supposed to carry a man, he could give to it the required direction, bring it afterwards gradually into a horizontal position, and finish by inclining it upwards, by which the whole momentum of the machine would be brought to act on the air, and it would then probably reach nearly the same elevation as that from which it descended, besides having made some progress forward. But to obtain the greatest extent of

progressive motion the horizontal position should be maintained as long as possible, and the velocity increased when required by making another dip downwards. Supposing the weight of the machine and man to be 300lbs., and to be launched into the air at an elevation of 1000ft., there would thus be developed from gravity alone a force equal to one horse during the descent, provided it were made in ten minutes, or ten-horse power if made in one minute. Here, then, is ample force to commence with—one that weighs nothing or costs nothing. The bi-plane could be elevated to the required height by giving to it a rapid horizontal motion, which could be done gradually with a rope and windlass, and the horizontal motion afterwards changed to one inclining upwards; or it could be elevated in a breeze, like a kite, and released from the rope when a sufficient height had been attained. I think, however, that soaring birds are greatly aided by the unequal or constantly varying velocity with which the wind blows, because if the line *a* be supposed



to be the plane of a bird soaring, say at an angle represented by a rise of 1 in ten, and with the wind blowing from *b* to *c*, it is evident that any sudden gust would have the effect of lifting it upwards with a force ten times as great as it would drive it backwards, and the consequent increase in elevation would be so much power gained, which the bird would not fail to turn to the best account, after reaching its maximum height, by gliding down again when the gust was over and using its falling weight instead of muscular power to regain its forward position. On the other hand, if the velocity of the wind were quite uniform it would soon impart its complete motion to the bird, and the two would then form one moving mass.

Sir George Cayley thought that the soaring of birds is

due to the upward currents of air reflected from cliffs, hills, &c. That such currents exist to some extent there can be no doubt, although I have never heard of their effects having been felt by aëronauts in the descent of parachutes on a large scale, although pieces of paper and other light substances are often lifted and borne away by them, or at least by some whirling or other motion of the air, and in rare instances heavy bodies as well. It is possible that the direction of the wind may not always be quite horizontal but slightly inclined upwards, in which case there would be no necessity for any inclination in the body of the bird. By experimenting with the bi-plane in soaring the art of aërial balancing would be acquired, which is very important. But it will doubtless be asked, can it be made large enough to carry a man and yet weigh no more in proportion to its surface than a light bird, say about 1lb. to every square foot of surface. My opinion is that it can, although there are some circumstances that tell against that opinion and some for it. I will state them very briefly. The weight of any framework, when the same proportions are adhered to, increases according to the cube of its dimensions, and the strain upon it in the same ratio as the dimensions, so that by simply augmenting the lines of a structure ten times we increase its weight a thousand times, and it would also be, when so augmented, relatively only one-tenth as strong. On the side of gain, however, there are the following facts. The surface of an aëroplane increases according to the square of its dimensions, but practically the sustaining power is more, for, according to Hutton, large surfaces resist the air more in proportion than small ones, so that by increasing the dimensions ten times we increase the sustaining power considerably more than a hundred times; and as regard the framework, it is more easy on a large scale to avail ourselves of those resources which exist for strengthening it than it is on a small one, such as by making it tubular or cella-tubular, &c.

In conclusion, I have only to state again that all experiments involving human or other life should, for greater safety, be made over water.

At the conclusion of the Paper

The CHAIRMAN said the Author had directed their attention to the subject generally, and they would now be glad to hear any remarks.

Mr. SÉNÉCAL observed that it was of great importance that aërial apparatus should be able to come down vertically and gently, otherwise a model, going at some velocity, instead of coming down in one corner of the room might come down in another.

Mr. BROWN: The velocity can be instantly stopped by giving an angle up. If a bird wishes to stop he gives a little angle up and stops instantly. One plane is before the other: they are not side by side.

Mr. SÉNÉCAL: It is important to attend to the point of safety in coming down.

A Paper by Mr. Armour, C.E., on "Air Compression under Wing-planes," was read by Mr. F. W. Brearey, the Secretary of the Society, who stated that the Author was prevented from being present at the Meeting by an engagement at Newcastle-upon-Tyne.

AIR COMPRESSION UNDER WING-PLANES.

Within itself, when unresisted, the air in motion in a passing wind is of the same density as neighbouring air at rest. In gusty weather, when smoke or vapour is in the air to reveal the motion, we can see that, though making quick whirling changes, the air ever moves in buoyant bulk; and, neglecting the elastic pressure that must occur between two whirling

volumes that blend together with unequal motion, the vapour forms, however ragged and shredded out, show no indications of varying density anywhere, for the lightness and elastic buoyancy and freedom is uniform throughout, and the ragged vapour forms and the clear air between are carried by the eddies equally.

This uniformity of density, however, is at once broken when a wing-plane is presented to the current. Against the windward face the air compresses itself by the force of its momentum, and thereby forms a cushion of resistance. In the time of the compression the weight of air compressed has expended its motive force, and the force of the impact, whether derived from air thus in motion against the wing, or from the wing beating against still air, determines the value of the support that the wing receives.

The normal pressure of the atmosphere is 15lbs. per square inch nearly. The mechanical pressure of 1lb. per square foot of wing-plane is only about 0·007lb. per square inch, or about 1-2140th of the normal pressure; and as the elastic pressure of air is according to the density, the air come upon by the wing-plane would give the required static resistance of 1lb. per square foot to the passing body, if enduring compression to the extent of 1-2140th of its normal volume.

The air under a bird's wing, however, being free for displacement on all sides, we cannot argue for it as in the case of air enclosed, and are at present unable to determine the depth of volume that would be active in resisting the displacing pressure in the time the plane would be in contact, and are free to reason only that the pressure is made sensible simply by the resistance offered by the inertia of the air beneath it, and that this force of air-inertia is equal to that of the weight of pressure in the plane.

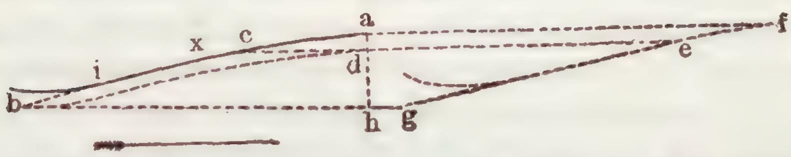
The form of the wing, however, with its lateral extension

and its forward motion in flight, allows to the air come upon only the downward direction for the motion of displacement during the short time of contact; and in the elasticity and ready compressibility of the air do we look for the compression needed for the support of the passing weight, and believe that this compression must be experienced by the air in immediate contact with the plane, in the act of transmitting the pressure to the outlying weight of air; and we consider, further, that the quicker the velocity of the plane the more compact will be the volume of compression.

Thibault, experimenting with forms square, circular, and triangular, propelled with their faces perpendicular to the direction of the motion, so that the whole area was pressed equally, found the resistances were independent of the form he had given to the area. In the question of mechanical flight, however, we have to deal with angular pressure, in which it is evident that the forward and the rearward surfaces are acting under different conditions on the air.

The value of the sensible pressure for the sustenance of free weight cannot be estimated irrespective of the time occupied in its development upon the air. In the case of any given force of inertia developed in air, and in sustaining balance with the imposed pressure, any slight addition to this imposed pressure, requiring corresponding increase in the sustaining force of inertia, acts as free or unbalanced motive force until the additional resistance needed for its sustenance is developed in the air.

Fig. 1.



In Fig. 1 let *ab* represent a plane measuring 1 foot square,

and the forepart of it, *ax,* the 3 inches breadth of a narrow plane, with motion in the direction of the arrow.

The plane must travel a certain distance *cd,* in imparting the motion of compression *ad,* and in travelling further, say the distance *ce,* we may regard the surface volume *acef* as receiving the motion of direct compression; and as it is clear that the act of direct compression by the plane, ends when the sustaining density has been reached, we have this compressed air interposing in the manner of an elastic cushion between the plane and the body of air beneath, so that the body of air beneath can receive displacing pressure through the cushion only, the density of the cushion not being increased by this duty, but may rather be diminished, because, in the time *dh* the pressure is tending to put the bulk of free air beneath in motion bodily, the rearward area *cb* seemingly acting mainly as an abutment to the elastic cushion pressure, which is thus acting freely outwards upon the air beneath, say on the short line *dh,* while the plane is advancing on the longer line *bh.*

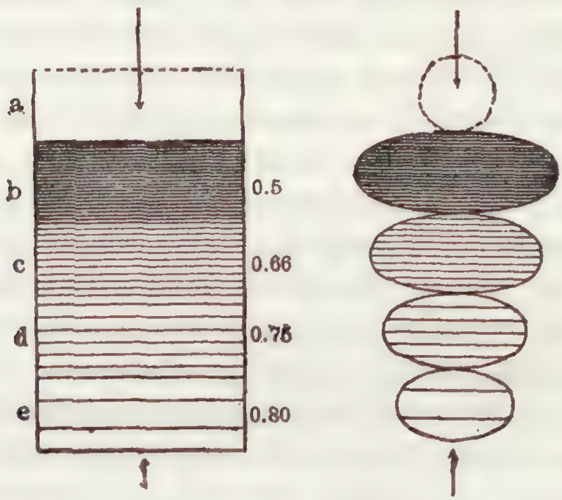
If *ah* were air come upon at rest in a confined space, and suddenly compressed to the extent *ad,* the sum of the dynamic energy that would be expended in the act, first, of compression to the required density, and then, at the point *d,* of starting the compressed weight at the velocity of displacement assumed for the plane in constant motion, would amount to more than the weight of the quiet force that would be sufficient to maintain the compression at the point *d,* but the difference in the greater amount would have expression in the motion of displacement of the air beneath; and if this quiet force be represented by the pressure due to the density of the cushion of compression, we have in the cushion *acef* work equal to the dynamic energy expended in the compression, acting uniformly through the time *ce*; and in the volume *cbge* the elastic pressure due to the density of the compression maintained

by the continued advance of the plane, but doing work in the displacement of the bulk of yielding air.

The surface volume of compression *acef* is seen in Fig. 1 to be common to the two planes *ax* and *ab*, but experimental data is yet needed to determine the rate at which the expansive pressure of the cushion diffuses itself with displacing force through the bulk of free air beneath; and, likewise, the limit to which, with a given velocity and angle of inclination, the narrowing of the wing-plane may be carried without loss of effect.

The depth, as shown in Fig. 1, for the volume of compression is purely arbitrary.

Fig. 2.



In Fig. 2 let *a*, *b*, *c*, *d*, and *e* represent five distinct volumes of air at normal density, that may be acted upon in successive order and together. First, compress *a* and *b* into the single volume *b* and call the double density 1. Next, let this double density relieve itself by thinning into the successive volumes *c*, *d*, and *e*. We will thereby have the ratio of volume to the

original double density 1, in these successive enlargements of volume, in the order,

b	*c*	*d*	*e*
0·5.	0·66.	0·75.	0·80.

Now, assuming these ratios of varying density in successive strata to occur in the cushion and the body of air immediately beneath, at some point below *d*, Fig. 1, with the greater density *b* in contact with the plane, we would have the force of inertia in the successive strata developing with the velocity in proportion to the displacing wave pressure that decreases outwards towards *e* in the yielding body of air, and would have a wave of pressure *e* keeping, roughly speaking, about the same relative distance in advance of *b* in the transmission of the pressure through the body of air that has to be put in motion; but as the density of *c*, with its less fully developed force of inertia, is only about 0·75 the density of *b*, the tendency here would be for *b*, with its maximum expansive force, to expand into *c* in advance of the motion given by the plane, and similarly with regard to the other volumes *d* and *e*.

We would thereby have the density next the plane, say at some point, *i*, Fig. 1, thinned to less than the density that obtained when the force of inertia of the weight of pressure was started with the given constant velocity at *d*: that is, the pressure *b* of Fig. 2 would be partially converted into motion in *c*, while the more distant effect in *e* would be modified by the loosening of the air about the rear of the plane.

The work done through the cushion can be no greater in amount than the work thrown into it by the plane, but the act of compression and the starting into bodily motion of the compressed volume of resistance near the forward edge, before the imposed pressure has been transmitted far enough to put the bulk of air beneath in motion, gives to the forward area of the plane more duty to perform than is done by the rearward area, which

comes upon air that is already started, so that the work to be done by the rearward area upon the body of air already yielding beneath is, in amount, only the difference between the force of resistance already developed in the yielding body by the forward area and the displacing force.

In a stationary stroke, say of the wing of a bird that does not leave its perch, resistance is developed on a surface of air equal simply to the area of the wing; whereas, in flight, resistance is developed on an extended surface equal to the length of wing from shoulder to tip by the distance travelled in a given time.

In the stationary stroke we have the wing-pressure maintaining, in continuous manner, the definite wave of displacing-pressure, which was originated and put in motion by a definite effort, say at the beginning of the stroke; whereas, in the stroke made by the same wing during flight, though the constant area of resistance be the same as in the stationary stroke, the wing in a given time has had to expend force in developing a wave of pressure from an extended surface area equal to the space that the wing has travelled over in that time, the energy and weight of the respective stationary and flight waves being determined by the velocity of the respective strokes.

In the formation of the cushion of resistance in new air, with the wing coming with sustained constant velocity upon air assumed to be at rest, we have the resistance of the air's expansive force developing with the growing density under the mechanically-applied compressive force of the wing, and the suddenness with which the force acts, confines the direct effect of the impact to the air more immediately in contact with the wing face; but, simultaneously with the motion of compression that forms the cushion of resistance on the front face of the wing, we have displacement motion developing in the rear, the air there

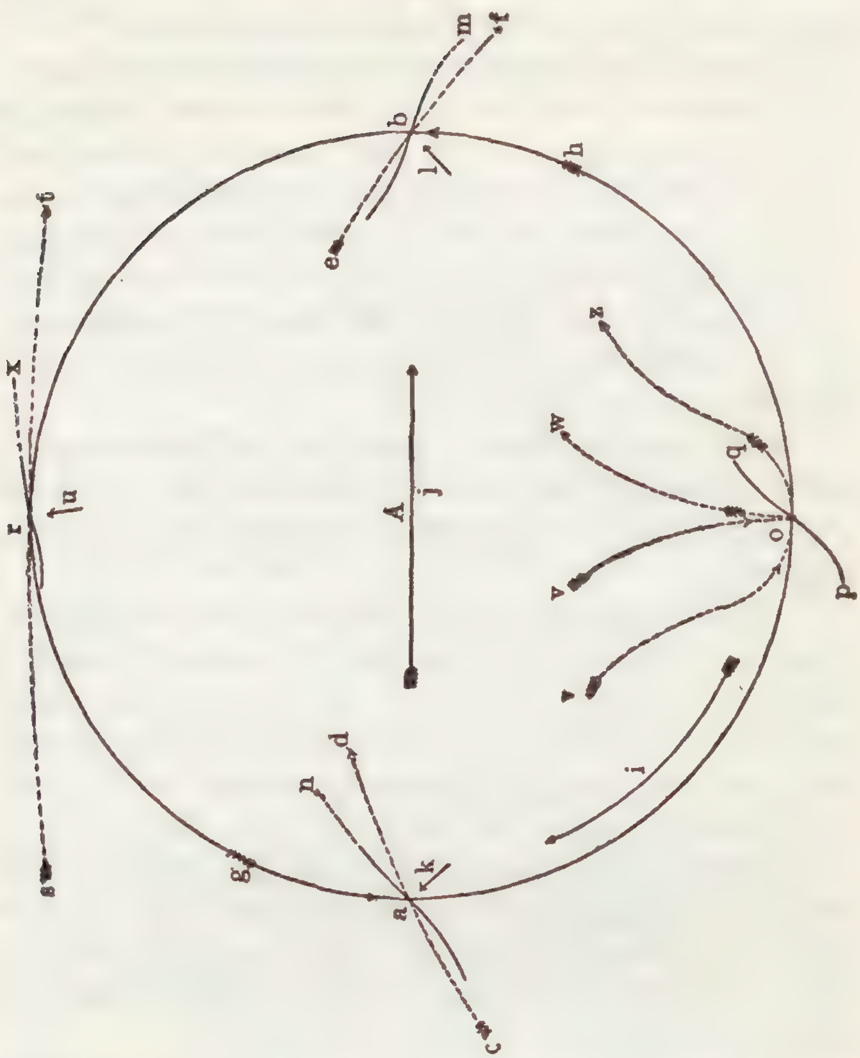
moving, say, laterally inward behind upon the wing, the cushion of resistance when formed acting upon the body of air beneath it in front and displacing it, say, laterally outwards to take, it may be, in the case of the stationary stroke, the place of the air that is thus closing in behind ; and as all this lateral movement in both front and rear is produced by the action of the wing, and as the lateral motion is motion of displacement due to the action of the wing, we have, once the cushion of resistance is formed and in motion, the displacement-resistance represented by the sensible pressure due to the density of the air that forms the cushion.

In the case of a wing in flight, however, the lateral motion of displacement outward from the front face and inward upon the rear face will not occur in the balanced manner we have spoken of with more immediate reference to a stationary stroke, or to a plane falling vertically, as the air displaced from the front face of the wing in flight gets clear only when the wing has passed beyond. Moreover, any tendency to a partial vacuum on the rear face of the wing would cause the wing to flatten to the direction of flight under the sustaining pressure developed on the under face.

To produce the up-and-down motions of a wing stroke it has been suggested to rotate the wing-planes round a horizontal axis. This would give velocity of rise equal to the velocity of fall; whereas, in bird flight, according to Marey, the rise is quicker than the fall.

In the case of a bird, however, the rise and fall occur alternately, whereas, in a rotating series of planes rise and fall occur together in continuous action.

Fig. 3.



In Fig. 3 let *a* and *b* be two wing-planes rotating round the axis *A*, in the direction indicated by the arrow *i*.

It is apparent that if the centre *A* be stationary the downward resistance *ga* will be in balance with the upward resistance *hb*, when the planes are in the relative positions shown, and the motive power exerted through the axle *A* will

have to overcome resistance $ga + hb$, with leverage Aa, and the gravity of the dead weight will be unsupported.

If motion of translation, however, be given to the centre A, in the direction of the arrow j, we then have the wing-planes a and b moving in a curved path indicated by the curved arrows *cd* and *ef*, with the resistances perpendicular to the face of plane roughly indicated by the arrows k and l, both acting in the support of the dead weight, while the motive power exerted through the axle A will have to overcome only the angular resistance to progression due to the angle of inclination on the path *nad* or *mbf*.

The difference between these two angles as drawn in Fig. 3, however, in relation to the horizontal line j, gives to k a backward tendency similar to the tendency in a bird's wing when rising.

This, as in the case of the bird, will act in slowing the velocity of translation j, and thereby will increase the angle of inclination of the curved path at a and b; but as the velocity of rotation, i, is assumed to continue constant, and as the plane will not be free to oscillate farther than shown at b, we have on the shortened path j, and consequent greater angle *mbf*, the plane b exerting maximum lifting or sustaining pressure, while the plane at a, being free to oscillate at the angle shown, is rising with a flatter angle *nad;* and as we assume the rear edge to be elastic, it is evident that the rearward curve at b will act in favour of the velocity of translation j.

Each wing-plane, in rotating order, will form a surface of resistance for itself in advance of the one preceding, the distance in advance being determined by the velocities i and j.

If the velocities i and j are equal, as in the case of a wheel running on the ground, then the wing-plane, gradually slowing on the path *vo*, is for the moment stationary at the lowest point o, the loss here, however, being recovered at the highest

point of rotation r, where the wing-plane, with flattened angle of inclination xrt, is in motion with velocity $i + j$.

When the motion j is in excess of the motion i we will then have the plane descending on the curve yo and rising on oz.

The spindles on which the wing-planes oscillate in assuming the required angles of inclination are supposed to be kept by mechanical means in connection with the axis A, so that the wing-planes balanced on them shall always have the same edge leading, and to allow of the oscillation the wing-plane may be not keyed to the spindle, but connected to it by a light spring, which may be, say, of spiral form, concentric with the spindle, and which, when the wheel is at rest, would keep the wing-plane uniformly at the easy angle shown at the lowest point o.

The rearward part of the plane op would here require to be slightly greater than the forward part og, but the plane would be so set on the spindle, that when at the point b the excess area of op would be greater than when at the point a, so as thereby to neutralize the greater effort of the restraining spring at b.

On a former occasion, when the forces at work were more imperfectly comprehended than now, we spoke of planes rotating in a wheel that made its starting run upon the ground; but, to get the required starting pressure on them to give sustenance to the weight that will have to be carried, their motion of rotation at the first will have to be quicker than running contact with the ground would admit of: better to launch out as a bird does from a perch.

Further, two systems of wing-planes in wheel form, one placed in advance of the other with the weights and motive power seated between them, would certainly be found preferable to a single wheel with the load within it.

After the reading of the above

Mr. MOY said that he thought that it was due to the Society that any Gentleman sending a Paper of this description should send also a model to explain its action. The action of the proposed plan was somewhat like a paddle-wheel of a steam vessel, with floats of a wave-form intended to act upwards both in the downward and upward motion. With respect to the wave-form of aëroplane he believed he was the first to propose that form. In doing so he had drawn his conclusions from ship building experience; but he had since then modified his views, as ships had to provide for the closing in of the water at the stern after moving the water laterally. But the wave-formed aëroplane might well be cut in half, leaving the air in motion to take care of itself instead of providing for bringing it again to a state of rest. But as such coarse angles as those shown in the drawings were quite out of the question, and very fine angles must be used, the flatter the aëroplanes were made the better.

Mr. BREAREY: After what has been said about inclined planes I am plainly inclined to say they should be quite flat. (A laugh.)

Experiments were made in the room with a small aëroplane set in motion by Mr. Brearey, who introduced to the Meeting Mr. Cayley-Worsley, nephew of Sir George Cayley, whose experiments in aëronautics were well-known to most of the Members.

Mr. SÉNÉCAL enquired if there were any data on that class of experiments?

Mr. BREAREY: Yes; you will find it all in "Nicholson's Journal."

Mr. Moy read a Paper, as follows, in

REPLY TO SOME REMARKS IN THE "QUARTERLY REVIEW" FOR 1875.

My attention was lately directed, through the vigilance of our worthy Secretary to an Article in the "Quarterly Review" for 1875, on the subject of aëronautics; and as the Article is written, in some respects, with considerable ability, and yet contains, in other respects, some of the most popular fallacies in regard to mechanical flight, it occurred to me that a short Paper on the subject might be of use in correcting such errors and directing the thoughts of those who are working at the problem into the right channel.

The writer of the Article in question expresses himself as follows:—

"There are many students of aërial locomotion who profess a contempt for the balloon as a mere plaything, and consider that the only proper solution of the problem is by a flying machine which shall sustain itself in the air, like a bird, by mechanical means. They disdain floating power, which they say birds do not possess, and which is, therefore, unnecessary. It would be just as reasonable to propose, on analogous grounds, to abolish boats and substitute swimming machines. The '*plus lourd que l'air*' doctrine is a delusion founded on the mechanical blunder of confounding gravity and momentum, which are two distinct things. It is a more reasonable objection that a balloon, from its large size, must offer a great resistance to the air at high speeds, but this resistance has been enormously over-rated, and is a cheap price at which to acquire the first condition of aërial locomotion—that of overcoming the action of gravity. At all events a dirigible balloon is a thing actually in existence: a flying machine is, at present, only an idea."

When I had read these remarks in such a work as the "Quarterly" I drew a long breath and rubbed my eyes with surprise and astonishment, as I considered that every writer of ability had educated himself beyond this; but, as I find that such opinions are only too common, I will trespass upon your time for a little while in order to put the matter in a clearer and more correct light.

Although I am one of the workers at the problem of mechanical flight I have a large amount of admiration for balloons and the many able aëronauts who navigate them; but when I am advised to attempt to put balloons to a task that they are incapable of performing, I of course treat *such advice* with contempt (not the *balloons*, as the writer asserts). If I am content to go a short distance through the air, and in whatever direction the wind may happen to blow, a balloon will serve my purpose very well, and, under good management, it is doubtless a very pleasant and enjoyable mode of travelling; but if a definite course is desired, unaffected to any serious extent by wind, then the balloon certainly becomes a mere plaything, and recourse must be had to a mode of flight which balances the effect of gravitation in another way, and at the same time is capable of attaining a high rate of speed.

The writer in the "Quarterly" seems much taken with the result of M. de Lôme's balloon, and says, in a foot-note, that "the resistance to M. de Lôme's balloon, of 122,000 cubic feet at 5 miles an hour, was only $21\frac{1}{2}$lbs.; at 20 miles an hour it would be 344lbs." Very good indeed; but first of all the writer must not assume that, because this air ship could be driven at the extremely low rate of 5 miles an hour in still air, that therefore she could be driven at 20 miles an hour and keep her shape; indeed it is very doubtful. But, assuming that this could be done, let us see what power would be required.

If a steam ship requires a pull of 344lbs., the engine must

give out an indicated power which would, by calculation, be as 100 is to 45, and it would not be safe to reckon on less than 43 indicated horse-power for M. de Lôme's balloon at 20 miles an hour. The gas for this balloon cost £360., its capacity being 122,000 cubic feet and the gas pure hydrogen. Its floating power was as follows:—

	Tons.
Balloon, accessories, and instruments......	1·75
Crew of 14 men with baggage, &c.	1·13
Packages or cargo	0·27
Available ballast	0·59
Total.........	3·74

These 14 men only drove it at 5 miles an hour, and I have shown that 40 horse-power would be required to drive it at 20 miles an hour. We therefore require, in order to carry the Reviewer's ideas into effect, a very costly balloon, very costly gas, and a costly steam engine, in order to go at the miserable speed of 23 miles an hour; and if a contrary wind happened to blow at that speed all this costly apparatus would be required merely to go nowhere, but just to hold your own like a steamer on a lee shore in a gale.

I must also repeat the fact that balloons will not keep their shape when opposed by a current of wind, and are very much given to "tearing" operations.

I think I have quite justified myself in treating balloons as mere "playthings, and disdaining floating power which birds do not possess." I have also shown that the resistance to balloons at high speeds has not been "enormously over-rated," for 20 miles an hour is an absurdly low speed, and at that speed the game is not worth the candle, and a higher speed is impossible with balloons; in fact I do not believe that any balloon could be driven at even 20 miles an hour.

Even if I allow that a 40 horse-power engine could be safely carried by M. de Lôme's balloon, and that a journey of 1000 miles can be performed at 20 miles an hour, and suppose also that it can be performed in a dead calm, she would require fuel for 50 hours steaming, which, as it would be impossible to carry, she would want a number of coaling stations. I need scarcely proceed any further: the whole idea is absurd and impossible.

Secondly—The writer of the Article says: "It would be just as reasonable to propose, on analogous grounds, to abolish boats and substitute swimming machines. The '*plus lourd que l'air*' is a delusion founded on the mechanical blunder of confounding gravity and momentum, which are two distinct things."

If we could *breathe* water instead of air the very best mode of navigation by water would be after the style of the torpedo boat, *under* water—below the surface—and not upon the water; but as it so happens that water navigation is effected in two elements, air and water, we *must* go *upon* the water instead of *under* it. There is no analogy whatever and no parallelism in the argument of the writer of the Article in question. We cannot go *upon* the air; we must go *in* it. We can only go *upon* the water; we cannot remain *in* it.

Again; we find an abundance of practical illustrations of living things *in* water, sustaining themselves both by displacement and by swimming, as cod, mackerel, &c., by displacement, and soles, plaice, &c. by swimming; but *in the air* we do not find *one* living illustration of the author's flotation theory, and the reason is obvious. Water is 800 times heavier than air, and therefore the bulk has to be increased 800 times; and as there is a wonderful power in the air to sustain rapidly-moving planes, we prefer speed and thereby promote economy.

This brings me to the third head of my subject, namely,

that I consider (taking the words of the writer) "that the only proper solution of the problem is by a flying machine which shall sustain itself *in* the air, like a bird, by mechanical means."

It is now eleven years since I published an Article in the "Mechanics' Magazine," showing that high speed could be obtained with sustaining planes without requiring greater power than low speed, and not only so, but that the higher speed was the most economical in every way. And in that Article I also mentioned that, without a considerable expenditure in money, the problem would never be solved. I consider that I have some right to speak thus, having spent a large amount of money and time in arriving at the highly-favourable results which I obtained twelve months ago, when a 3-horse engine lifted 120lbs. dead weight, in the presence of some of the most distinguished members of this Society.

Now, I will take the propelled aëroplane without gas, and, taking the total weight at half-a-ton, 1120lbs., I will allow an aëroplane surface of 90 square feet, placed at an angle of one degree from the horizontal. The resistance would be 0·375 of a pound to the square foot, which, multiplied by 90, is equal to 33·75lbs., the speed would be 200 miles an hour, and the vertical thrust 12·5lbs. per square foot; then

$$\frac{17{,}600 \times 33{\cdot}75}{33{,}000} = 18 \text{ horse-power.}$$

The engine power would require the same margin as before, say 30 horse-power to effect the thrust of 18 horse-power The weight of engine need not exceed 600lb., and fuel, aëroplanes, frame, &c., with two persons, would all come within 1120lbs., and be capable of going 200 miles an hour. Now, suppose the journey to be 1000 miles, as before, the cost of the fuel would not exceed £2. No £360. worth of gas has to be supplied to the aërial steamer; no damaged silk to be repaired;

no ballast required; a high speed attained so as to render the game worth the candle; a distance of 1000 miles accomplished with an expenditure of about £2. in fuel; the course chosen with certainty, speed, and precision; repairs and renewals of a very economical description; and safety far exceeding that of any railway train.

In conclusion, I beg to say that I am not guilty of the mechanical blunder of confounding gravity and momentum. I know that they are two distinct things, and I have not overrated the resistance due to gas bags at high speeds; such resistance increases about as the square of the speed, and is capable of flattening the gas bag so as to reduce its contents and compel it to take the most disagreeable course to aëronauts, that of *vertical descent,* for as soon as a balloon is put out of shape it loses in capacity and consequent ascensive power.

Mr. Moy illustrated his paper with a wooden screw, worked by clockwork, the surface of the two planes being 11 sq. inches, which, when set in motion, lifted a small weight, and was made to travel either to the right or left in a circle by inclining the orbit. He then, with another apparatus, exhibited planes moved rapidly in a circle, which, although quite free to fall, were sustained by the motion and upward pressure of the air, and were quite incapable of falling while in motion.

Mr. Moy was also glad to be able to say that, by the kindness and courtesy of his old friend Mr. Coxwell, who was present and who had favoured him with the loan of two small balloons to illustrate his subject, he could show them the absurdity of attempting to drive balloons from the car, and, by waving one of these to fro, it was seen that this often-proposed idea is utterly useless; but he shewed, at the same time, that vertical assistance to balloons by means of screws was quite feasible and practicable, preventing much expenditure of ballast and gas.

In conclusion, Mr. Moy informed the audience that his patent steam engine was a commercial success, and although he invented it for aëronautical purposes he was applying it to other uses, and he was now having one made for a launch, which would have a heating surface of exactly 100 square feet, and from this he would be able to obtain valuable data, and he then intended to make a 30-horse engine which would take up two aëronauts vertically, and when the vertical ascent has been obtained the horizontal movement would be an easy matter.

Mr. Brown said he would not go so far as Mr. Moy against the balloon. Mr. Moy's argument was against the globular form of the balloon, but there was an observation made by Mr. Moy which completely destroyed a great part of his argument. Mr. Moy said it was very strange that a weight completely immersed can be propelled better than when partially immersed.

Mr. Moy: A torpedo goes under water with greater ease than it would on the water.

Mr. Brown said that the deeper it was immersed in the water the quicker the water closed behind it, so that fishes swam better the deeper they were down. That was an argument which told in favour of balloons and against ships. If a ship was made round she would be quite as unmanageable as a balloon. The proper form for a balloon was an acute angle in front, disregarding altogether the angle behind, because the velocity with which the air closed a vacuum was about thirty times greater than water. Therefore they had the advantage in the balloon of making their cleaving angle the whole length of the balloon. In a ship they could not do that. The body of a bird formed an acute angle. His beak, head, and neck cleaved the air in a most admirable way. Therefore in the balloon they should have a most acute angle so that the air might not press but might glide off. If their angle was a

certain length, and they made it twice as long, the resistance would be diminished one-half, although the surface was increased. When the angle was made very long the resistance would be reduced almost to zero. He had great hopes that this theory would be brought into practice.

Mr. Brearey read a communication from Mr. Artingstall, of Manchester, as follows:—

"In the last Letter I wrote to you, that was inserted in the 9th Annual Report of the Aëronautical Society, I denied the almost-universal opinion, even of eminent mathematicians, viz., that the resistance of air is as the square of its velocity, and stated that the theory of the impact of military projectiles was much nearer the truth. A *single* bullet or solid particle is as the square of its velocity, but a stream of bullets or solid particles, like a stream of air, would be as the cube of the velocity, assuming that the whole of the momentum is expended on the target or surface.

"All matter, whether solid, fluid, or gaseous, *theoretically*, is subject to this law; but if a stream of air is directed against a surface a great part of its momentum is dispersed sideways, in what we call 'slip,' rather a vague word, but perhaps it depends upon the fundamental law, viz., that all free motion takes the path of the least resistance; however, one thing is certain, that slip is, beyond comparison, the most difficult subject to deal with in aëronautics, yet this property is highly favourable to progressive flight, for all bodies shaped like a bird, and moving in one continuous direction through the atmosphere, experience comparatively very little resistance, but surfaces vibrating in a peculiar manner lay hold of the air powerfully, hence we see the wonders of flight achieved by this combination of the minimum of resistance with the maximum of propelling power, for there is no real slip in true

flight. How is this wonderful vibration accomplished? I believe that the double vibration of the wing (that is the up-and-down stroke) is excited *chiefly* by the single pull of the great pectoral muscle, and not only so, but the buoyant or propelling power is all transferred to the under side of the wing and maintained there *without cessation*, also the bird or bat progresses *steadily*, notwithstanding the wings may not be propelling in a line with the centre of gravity.

"The following may bear on this subject. In the Letter just referred to I called your attention to a curious effect produced by suddenly cutting the air with the thin edge of an elastic wing, thereby producing a singular pulsation. Since then I have experimented farther. Instead of merely striking the air a single stroke I fixed a light and strong artificial wing at the end of a round and slender rod of highly-elastic steel, thus,

A

and gave it a circular motion impelling the thin edge *A* against the atmosphere. A series of beautiful and rhythmatic pulsations took place with powerful hold upon the air; in fact a buoyant rotary and vibrating wing (not a screw) but in theory of action resembling a bird's or bat's wing. It must be understood that the circular motion was a substitute for a blast of air.

"But to modify this experiment and make it more nearly resemble a natural wing, I took the wing and its steel rod from the rotating machine and exposed it to a very strong wind on an elevated situation. It vibrated similar to what it did in its 'orbit' motion, but in the wind its curves of vibration could be much better observed, yet it was too quick to be properly traced by the eye. A larger and consequently a slower moving wing must be constructed, but so far the results are very encouraging, and I conceive may be practically adopted.

"When my experiments are more advanced I may make them the subject of another Paper.

"There is every reason to believe that *long and narrow*-winged birds, such as the albatross, swift, common swallow, &c., when they have acquired their initial velocity, go through their wonderful evolutions, in a *perpendicular* mass of air, with comparatively less expenditure of power than a good skater does on a *horizontal* plane of the best ice and with far grater speed; in fact it appears as if the long-winged birds have very little more to do than first acquire momentum and then steer it. At all events very little power is required to overcome the resistance of the atmosphere to progression, and, theoretically, the momentum lost in ascent is nearly regained in the succceding descent, and rapid ascent is generally accomplished by swift birds quickly directing their momentum upwards, and not by great labour as the sparrow reaches the house-top.

"I may, in conclusion, remark that all artificial vibratory wings, like those of the bat (hitherto constructed), require enormous power compared with the small weight raised and the short duration of flight, my experiments mentioned in the Annual Reports of the Aëronautical Society for 1866 and 1868 being no exception; therefore we must endeavour to improve the vibrating wing by ascertaining its true principle of action."

Mr. F. Cayley-Worsley said it struck him that the very nicely-adjusted science they were engaged in, required very careful experiments, and he did not hear that those experiments had been made at all on an efficient scale. He had made many experiments as long as the power lasted for the balancing, steering, and adjusting of aërial apparatus; but to set a steam engine going without giving adjusting power, must result, as it seemed to him, in utter failure. He had seen many of the experiments of Sir George Cayley, who was

called the "flying baronet," and he was convinced from these that they must have a convenient experimental power. In his opinion there was a means of getting experimental power without going to the expense of steam engines or any large machinery, and the question was whether the Aëronautical Society was inclined to go into anything of that kind? He believed compressed air could be used as a temporary motive power. If they could get power to go 100 yards they could easily get power to go beyond that distance. He did not see in what other way than by experiment they were to succeed with a machine which required the marvellous adaptability of a bird. He should like to see a compressed air engine made which would give an eight or ten minutes' run; then the engineer would be able to take data. Without data it was an extremely difficult question which they had to contend with. The engine made and exhibited at the Crystal Palace appeared to his mind to have sufficient power to move off the ground; but, to a moral certainty, it would have turned head over heels. Well, did it not follow that, unless they had carefully adjusted the centre of gravity they would certainly come to smash? (Hear, hear.)

Mr. Moy said he could assure him (Mr. Cayley-Worsley) that he had paid great attention to the subject of balancing. It would be very foolish not to do so. In the experiments at the Crystal Palace the friction on the ground was so great that they could not get more than twelve miles an hour in a horizontal direction, and, therefore, it could not rise; but when they tried the vertical movement they got a lift of 120lbs. with 3-horse power. He did not see why a 30-horse engine should not be made right off. He had seen many miserable attempts made with springs, but just as the machine was rising, the power was gone and the machine stopped. He maintained that a 30-horse engine properly applied to aëro-

planes would raise not only itself but two aëronauts. When such an apparatus could go straight up and come down as gradually as it went up, then they might talk about horizontal movement. He quite admitted that care was required in balancing, and balancing should certainly receive the greatest attention after the necessary power for the upward movement had been obtained.

Mr. Brown: As regards the difficulty of balancing and control I think I have completely cured that.

Mr. Moy: Why did you not put an engine to your model?

Mr. Brown replied that it had only been tried on a small scale. It always came down in the same way that it went up. Of course he considered the balancing the most important part to deal with, and they could not proceed further until they had mastered that which he believed he had done. Still he should like to see it tried on a large scale. He should like to have to contend with unequal currents, which they could not find in a room.

Mr. Sénécal remarked that whatever experiments were made there certainly must be a man to guide, and the steering apparatus must be independent of the power that moved the wheels.

Mr. Sénécal gave some notes on aëroplanes of different forms, some loaded with weights, which he illustrated with paper models.

He said that while planes of even width and thickness revolve upon their own axes, and their path of translation is rectilinear, the motions of triangular planes are much more complicated. These planes are obtained by dividing the circumference into blades of different widths. These blades, besides revolving upon their axis, rotate also round a vertical conic axis, whose base is upward, the vertex of the plane describing a spiral round the conical axis.

He found that the rate of revolution and rotation increases in direct proportion as the base and the length of the blade decreases, and the length travelled over in a unit of time decreases also in the same proportion. The shifting of the centre of ~~gravity~~ of these blades is most interesting. It was found that the centre of gravity of narrow planes was near the vertex and on the edge of the plane, but recedes towards the base and axis as it widens; it also travels from the axis towards the edge and vertex as the rate of revolution increases, and possibly that, at high velocities of rotation, the centre of gravity will be beyond the edge. The size of blade that revolves and rotates most steadily represents the 18th to the 24th part of the circumference. He also proved that by cutting a small plane out of the base it had the same effect as applying a weight at that point before cutting it. The plane will then revolve and rotate round with its base turned towards the vertical axis.

Mr. Sénécal then enunciated the following law: that planes, of whatever form, but of even thickness and rigid margin, in order to translate steadily, must carry their maximum load on a line representing the first 3rd part from the anterial margins of the plane; but one can, with impunity, apply graduated weights from that line right on to the edge, and, in some instances, a good distance beyond the edge, and high rate of speed is the result. The rate of translation increases directly with the load placed on the different points of the graduations from that line of the centre of gravity.

He also liberated several narrow strips of paper showing, while revolving, nodal and ventral sections similar to musical strings in vibration, the number of aliquot parts increasing with the length of the ribbons and disappearing as the width increases.

A Gentleman present, referring to Mr. Sénécal's experiments, called his attention to the fact that small objects in water sank very slowly and large objects rapidly.

The CHAIRMAN said there was one observation he would like to make with respect to the little machine which had, that evening, been sent through the air, but, as far as he had seen, its motions were somewhat erratic. Now he was quite confident that if the two wings instead of being on one plane, were more inclined to each other, it would not veer about as at present shown, but would fly steadily. If it found itself turning sideways one wing would counter-balance the other, and the machine would have a tendency to steady itself. The wings should, therefore, be put at an obtuse angle to each other. As all the communications which were put down for that evening had been made, he would invite those present to return their best thanks to the Authors for the several communications which had been made, and he trusted some of them would lead to practical results and so be conducive to the great end they all had in view.

Mr. MOY proposed a vote of thanks to the Chairman for his kindness in presiding over that Meeting.

The Motion was adopted with acclamation.

The CHAIRMAN said he was glad to have an opportunity, in the smallest degree, of promoting the object they all had in view and of furthering the interests of that Society.

The Meeting then separated.

THE POWER DEVELOPED BY BIRDS,

BY

A. PENAUD;

READ BEFORE THE SOCIÉTÉ PHILOMATHIQUE DE PARIS

IN 1876.

In 1866 Mr. Wenham* pointed out that birds whilst in rapid flight encountered at each instant a fresh undisturbed body of air, and dispensed less power in full flight than in hovering. M. de Lauvire showed also, about the same time, the advantage of the oblique action of surfaces upon the air, in taking for basis the experiments of Thibault.

I have been able to establish the very simple law of the resistance to flat surfaces moving at very oblique angles in a fluid, and I developed their result in 1872.

By introducing in my calculations the results of several observations that I have made upon the different species of birds, I determined very nearly the work dispensed by them in full flight, this work was equivalent according to the species and the size of wings, to the elevation of the weight of the animal from 20in. to 5ft. per second, and generally superior to 40in. for the large species.

My calculations, founded upon a number of concordant experiments and upon a series of observed facts and theoretical

* Mr. F. H. Wenham is one of the Council of the Aëronautical Society of Great Britain.—*Ed.*

considerations, since that time have been applied to other purposes. I will mention the remarkable experiments made by Mr. Froude for the English Admiralty upon planes gliding on the surface of the water, and some made by M. Marey with planes rotating on a stand, and also in direct translation along an iron wire.

After having determined the work dispensed by birds in normal flight with these calculations, and also by other and independent means, I thought it would be of great interest to know the maximum power that birds were able to develop for a given time.

They require in certain circumstances a superabundance of strength, and flying machines that may be constructed in the future will also require, in though a less manner, a store of power upon which they can fall back in emergency; great power will be wanted to start from the ground.

One of the cases in which birds develop considerable power is, when they ascend almost vertically from the ground to a high perch, and it is then easy to watch them with precision, and make some nearly exact estimates. The bird in these ascents appears to develop almost the greatest amount of power it is capable of, for I have often seen pigeons still young only able to reach half-way to their cot, owing to excessive fatigue.

They have at last succeeded by ascending in a spiral of considerable extent, thus augmenting the duration of their flight, but, at the same time, owing to the forward movement diminishing the amount of work developed per second.

This fact is often observed when the young pigeons have taken a bath and their body and especially the wings are charged with humidity.

In the ascent the total work developed by the bird is divided into two parts, the one fixed, that is the work of

elevation, the other variable and increasing with the time, that is the work dispensed in finding a support in the air.

It is thus to the interest of the bird to rise as quickly as possible, which it generally does even when under no sense of fear. Their velocity of direct ascent is always several yards per second.

I have been able to measure with considerable accuracy the direct velocity of ascent of the stockdove when rising to a perch 35ft. 4in. from the ground. The mean velocity of ascent taken from 8 flights was 9ft. 1in.; the mean of the two slowest flights was 7ft. 7in.; and of the two most rapid 11ft. 7in. The ringdove gave a mean of 9ft. 11in.

I have made 15 observations of sparrows whilst mounting from the ground to a wall 28ft. 1in. high, and found a general mean velocity of 11ft. 3in.; mean of the two slowest 9ft. 11in., of the two fastest 14ft. 10½in.

This is the mean velocity in an upward flight of consider able length; but we must remember the bird starts without velocity, and settles in the same condition, so that in the middle of the flight the velocity is vastly greater than the above figures show.

If we assimilate the movement of the sparrow to that of a pendulum, which is plausible enough, we find the velocity in the middle of the ascent would be 16ft. 6½in.

For peacocks, which are heavy birds, who came every evening to pass the night upon the same tree, I found a velocity of 8ft. 3in. They rose in 2·6 seconds to a branch 21ft. 6in. from the ground, and in starting helped themselves by a vigorous stroke with their feet.

Amongst the birds that mount most rapidly are partridge, wild turtledove, and snipe, which are of moderate size, but provided with powerful pectoral muscles and small wings.

The sea-pie rises more rapidly still, and in some observa-

tions, not very exact owing to absence of convenient marks by which to measure the height, I found an ascent of about 20ft. per second.

Thus, and apart from all theory, it is certain that birds are capable of developing momentarily a force corresponding at least—

For the Peacock,	to one horse-power for every	66lbs.
Ditto	Pigeon (Stockdove and Ringdove)	57lbs.
Ditto	Sparrow	$48\frac{1}{2}$lbs.
Ditto	Sea-pie	about 26lbs.

As before mentioned the work of elevation is not the only one the bird has to do; it has still to find a support in a fluid extremely mobile. When a bird rises vertically without any horizontal movement and maintains itself upon the same column of air it throws the axis of its body into an almost vertical position, and the direction of the motion of the wings is almost horizontal. The amplitude of the stroke of the wing is very considerable and embraces the entire circumference. In the pigeon the extremities of the wings are often heard to strike each other.

The change in the angle of the plane of the wing at each oscillation is very great and exceeds 90°.

The wing, conveniently twisted upon itself, acts upon the air with great power during the down stroke, after the manner of an inclined plane; in the return stroke the wing still acts upon the air, but to a less extent and with its superior surface.

It then forms inclined planes in contrary directions, and receives back the horizontal impulse given to the air by the wing in the preceding oscillation.

The wings act thus in a similar manner to the tails of certain fish, describing in the air put in motion a sinuous path in the form of very close spirals. It flies after the manner of a heliçoptere, its body held vertically representing the nave, and its wings the blades of the screw.

By means of these horizontal oscillations of the wing of great amplitude, the bird acts upon a column of air of the greatest possible section, and having for base the circle described around the body by the wings. Vertical oscillations, excellent in full flight because the translation constantly brings new stratum of air under the wings, would be very disadvantageous in an almost vertical ascent, for it would put into motion a column of air far more restricted. I have found that the bird in vertical flight and in hovering creates an almost uniform current of air by means of the rapid succession of the oscillation and intensity of the changes in the plane of the wing.

The section of the current is evidently the horizontal projection of the area passed over by the wings. I have confirmed this by making a pigeon mount in smoke and in a net, with large meshes, covered with light bodies such as down.

When the Sphinx is suspended over a flower for the purpose of getting the juice, the foliage immediately below is visibly agitated in a continuous and regular manner by the current of air thrown from their little wings.

By blowing some smoke through a small tube into the current its dimensions and regularity are rendered still more visible.

By waving transversely near a candle the wings of a bird, freshly detached, or artificial wings, a nearly uniform current is easily obtained, of which the extent can be measured.

When a helicoptere, or an artifical bird of my construction, is presented to the candle, a uniform, continuous, cylindrical current is produced without dispersion or centrifugal movement.

I have brought one of my helicopteres in order to make this experiment before the Society. It is seen contrary to the general opinion, that the air far from being dispensed at the circumference of the screw, tends to converge toward its axis, which is shown by the slight attraction of the flame.

Behind the screw, or for a considerable distance, the candle experiences only a feeble agitation as long as it is outside the cylinder, having for base the circle of the screw; but when it enters the cylinder it is violently blown. If the light is placed in front of the screw it is seen that the column is not continued, and that, immediately in front of the screw, a widely extending cone of suction is formed, which takes the air from all sides.

The same effects are produced by screws whose blades are inclined forward, and also when the screw is moved backwards or forwards along the column of air in motion. These facts show that the work dispersed by the bird, in order to find a support in the air which gives way beneath him with a velocity W, differs but little from the work necessary for maintaining an uniform speed W, in a tube having for section the horizontal projection of the area described by the wings of the bird.

In taking this last work as that of the bird, we shall be certain it is a minimum; for the uniformity of the movement of the air put in motion by the bird is not absolute, as there certainly exist eddies resulting from the impluse of the air directly struck by the wings acting on that more remote; but it is known that when a mass of fluid m with a velocity V, draws with it by lateral communication, another M at a velocity U we have $m \text{ V} = (\text{M} + m) \text{ U}$, and this formula, demonstrated by the experiments of M. Piarron de Mondiésir upon ventilation, by means of compressed air, involves the loss of momentum; but the loss of energy is small when M is mediocre relatively to m, and we have seen by its proved uniformity near the wing that such is the case with the currrent of air put in motion by the bird.

If P is the weight of the bird, l the length of its wing, ζ the arc described by the wing in the mean plane of its

oscillation, η the angle of this plane with the horizon, the section of the vertical descending current put in motion by the two wings will be l^2 arc ζ cos. η.

In order to get the volume acted upon per second the preceding expression must be multiplied by the length of the column of air put in motion during that time.

In stationary flight this length will be exactly the velocity W of the current, but in the case of flight ascending with a speed h, this ascent causes the constant creation of a current in a fresh body of air, owing to the singular form of the cone of aspiration, and the support is thus greatly increased.

It is in this manner that the screw of a steam-ship when acted upon by a constant force, turns but very little faster when the ship is going at full speed than when fastened to a fixed point; in f the first case however, the slip is less than $\frac{15}{100}$ whilst it is equal to the whole in the second case.

From this I think, in the present case, we may take for the length of column of air acted upon per second W + h (perhaps it would be better to take W + $f\,h$, f being a certain function of l, W, and h; but in the absence of exact data we will retain W + h).

This being settled, if π is the weight of the entire volume of air, and g the acceleration of the weight; the mass of air put in motion during one second by the wings will be

$$\eta = \frac{\pi}{g}\, l^2 \text{ arc } \zeta \text{ cos. } \eta\, (\mathrm{W} + h),$$

and the work expended per second in maintaining the current

$$\mathrm{T} = \frac{\mu\, \mathrm{W}^2}{2} = \frac{\pi}{2\, g}\, l^2 \text{ arc } \zeta \text{ cos. } \eta\, (\mathrm{W} + h)\, \mathrm{W}^2 = \mathrm{P\,W};$$

from whence we get

$$\mathrm{W} = \frac{1}{2}\left(-h + \sqrt{h^2 + \frac{8\, g\, \mathrm{P}}{\pi\, l^2 \text{ arc } \zeta \text{ cos. } \eta.}}\right)$$

The positive root alone must be used here.

It is seen that in stationary flight where $h = o$ the work varies proportionably to the power $\frac{3}{2}$ of the weight of the bird, and in an inverse ratio to the width across the wings (that is to say, the square root of the surface in the case of similar surfaces).

We will apply this formula to the ringdove, of which I have measured a great number, and found the several means to be $P = 480^{gr}$ $\quad l = 0^{m}, 32$ $\quad \zeta = 160^{\circ}$ $\quad \eta = 20^{\circ}$.

We have besides $\eta = 1^{k}, 24$ (mean condition), and $h = 3^{m}$.

It results that $W = 4^{m}, 1$ (a different method of calculation has given but slightly different results).

$\frac{h}{W + h} = 0{\cdot}42$ gives the thrust of the wings as elevators.

The elevation $W + h$, corresponding to the total work of support and ascension, is 23ft. 3in.

But we have not yet taken into account all the conditions of flight. I am convinced that the inertia of the wing, in spite of its marvellous lightness, absorbs a considerable amount of work. For want of time I will not enter into the details of these researches, but will content myself by saying that I have arrived at exact results by means of weighing different portions of the wings of birds and insects, by making an integral quantity of the moment of partial inertia with respect to the scapulo-humeral articulation, and by introducing the factors thus obtained in formula, taking account of the number and geometrical conditions of the oscillations of the wings. Applied to the ringdove these calculations give more than 6ft. 7in. to be added to the 23ft. 3in. already found.

This number which takes account of the useful absorption of the momentum by the resistance of the air at the end of the stroke, corresponds after a manner of its own to a maximum h forming part of it and W also, in a great measure, owing to the want of translation. In forward flight, the oscillations of

the wings being less rapid and numerous, the work of inertia is far less: moreover the absorption of the momentum by the resistance of the air is complete.

In the presence of this enormous figure of more than 6ft. 7in. I have been led to think that the elasticity of the wings and muscles play an important part at the end of the stroke, and that the wing acts as a spring similar to a tuning-fork in vibration. The admirable elasticity of the feathers and ligaments of the wings seem to agree with this theory. I have found, by experiment, that a feather is twice as elastic as steel, weight for weight. The muscles, when contracted, probably possess the power of storing up and restoring, to a certain extent, power like a spring. The work absorbed by the inertia is not wholly restored, and it is certain that it causes the total work of the ringdove to correspond definitely to a height of at least 26ft. 3in. per second, or 20lbs. per horse power.

I have arrived at results still more astonishing, in some calculations founded upon observations made upon the flight, at full speed, of the martin and sphinx.

Such is the maximum dynamic power that birds are capable of developing. It is considerable and very superior to that of mammalia and man in particular. It has not, however, happily any connection with the fantastic calculations of Navier. He dared to declare that the swallow flying at 50ft. per second developed power corresponding to an elevation of its weight to 948ft. per second.

Let us compare the power of a bird with that of man and the steam engine.

A man is able, during several hours, to climb a ladder at the rate of 6in. per second. The ringdove, which can fly also for many hours together, dispenses in full flight power equal to its own weight lifted 3ft. 7½in. per second. The proportion is thus 22 to 3.

By making a spurt I have found that a man can ascend to the 4th floor with a mean speed of 3ft. per second; but this experiment was made under disadvantageous circumstances. An athlete could do far more. If we compare this figure with the corresponding one of 23ft. 3in. total work expended by the ringdove in a vertical ascent, the proportion here is 7·9 to 1, and nearly the same as for the normal work.

The lightest motor that man has yet constructed is the non-condensing high-pressure expansive steam engine, such as express locomotives, steam fire engines, and the engines of fast steam launches. None of these weigh less than 66lbs. per horse power with only a very small provision of water and fuel; compound engines with surface condensers as at present used in war ships and mail boats weigh at least 275lbs. per horse power. In a flying machine the weight of the motor should never be more than a fraction of the total weight. According to my calculations it should not exceed ⅓rd, in order to leave sufficient weight for the supporting surfaces. Thus the actual motors we have, are far from equalling the power that the bird develops under certain circumstances, and even unable to develop the far less power that large birds expend in full flight for hours together by supporting themselves upon vast masses of new and undisturbed air.

Allow me, however, to express my conviction that, in the future more or less distant, science will create a light motor that will enable us to solve the problem of aviation.

LAWS RELATING TO PLANES GLIDING IN THE AIR.

BY

ALPHONSE PÉNAUD,

TRANSLATED FROM "L'AËRONAUTE" BY T. J. BENNETT.

I.

Newton, who was the first to study the resistance that fluids offer to a body moving in them, stated implicitly that the molecules of the fluid remained immovable up to the moment that the body touched them, and returned to a state of rest immediately afterwards.

He found that the resistance experienced by a flat surface was proportionate, 1° to its extent, 2° to the density of the fluid, 3° to the square of the velocity, 4° to the square of the sine of the angle of incidence, and 5° that it is normal to the surface. It was, however, soon discovered that this theory, altogether empirical, was often at discord with what experience taught, and a great number of experimental researches have been made at different periods in order to throw further light on the subject.

The result of these researches has been to prove that the last and second laws are in a great measure true, also the third, except for excessive velocities, as in the flight of a cannon ball; but the first law does not hold good except for surfaces of similar shape and position. The same surface experiences, other things being equal, a great difference of resistance, according to the shape of the body of which it makes part, and its position in that body.

The law of obliquity is altogether false. The results of experience differ greatly, sometimes less, but generally in excess of those given by the law.

As we have not been able to discover the laws of these complicated facts, we have thrown the anomalies presented between practice and theory upon the imperfections of the experiments made, and still continue to teach the laws, pure and simple, of Newton.

It was in applying them to the resistance experienced by the wings of birds that Navier (who besides was entirely ignorant of the mechanism of the wing) made his exorbitant calculations, and thus, in a great measure, was the cause of throwing aviation into the discredit from which it is now only beginning to emerge. These calculations were entirely contrary to facts, since, for example, he gives more than 12 double beats of the wings per second to the raven, who in reality only makes 3. A great number of daily phenomena have equally opened our eyes to the enormous resistance experienced by thin surfaces moved obliquely, viz.—the sailing of a ship close to the wind, the power given out by windmills, the thrust of a screw, the power of a rudder, which all give (quite an exceptional thing) better results in practice than in theory. Thus these laws of Newton are not universally adopted, especially in naval architecture, where views more in accordance with experimental facts are adopted.

It is acknowledged by those who have studied the subject of aviation that the bird develops vastly more power in hovering than in ordinary flight, when its wings attack the air at a very small angle, which is easily perceived by watching a bird coming directly towards you when only a little more than the edge of the wing is seen. This fact, the key of aërial navigation, has been noticed by several persons, amongst whom are the Duke of Argyll, M. de Lucy, and the Count d'Esterno, the author of the well-known book on flight.

Mr. Wenham, in his valuable Paper printed in the First Report of the English Society, has developed this idea, that flight is a phenomenon analogous to the collision of two bodies, and that the greater the mass of air attacked in a certain time the less will it be put in motion, and the work dispensed consequently less. He came to the conclusion that it would be advantageous to use a long and narrow surface like the wing of an albatross, moving rapidly at a very small angle, so as to act upon the greatest amount of air possible. He was thus the first to perceive the cause of the advantage of attacking the air obliquely, and the part played by the great spread of wing in the albatros and other long-winged birds, for if upon the spread of wings principally depends the surface of the stratum of air attacked, the mass of this air depends upon its thickness, which evidently diminishes with the size of the surfaces employed.

Lastly—M. de Louvrié, having carefully studied the subject, published several Papers, amongst which was one that appeared in *l'Aëronaute* for 1868, where he reproduced the results obtained by the most trustworthy experimentalists with surfaces presented at a small angle to the air, and insisted, and justly, that the results obtained by Thibault, which are indisputable, are the most applicable to flight.

The figures of Thibault show that for plane square surfaces the resistances normal to the surface remain nearly constant from 90° to 45°, and after that diminish progressively to 20°, from which to 0° it becomes sensibly proportionate to the simple sine of the angle of incidence. At 14° the pressure was about half of that experienced in the normal position, the speed being the same.

It was reserved for M. de Louvrié to bring to light these results and to demonstrate them to be in concordance with the flight of birds, and also show, by a rigorous analysis, the advantage of attacking the air obliquely.

II.

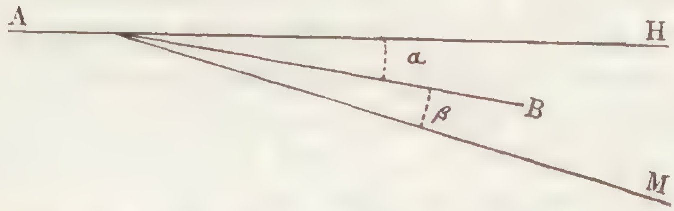
The above studies having opened up the subject, the Paper I have presented to the Society is a sequel to them.

As the surfaces of all flying beings, in spite of their variety, possess important points of similarity, we have used the laws of Newton, making, however, the resistance proportionate to the simple sine of the angle of incidence, which, as we have seen, increases rather too rapidly, except just for small angles with which we have principally to deal.

THEOREM I.

A bird that glides, falls the least possible distance when it employs for progression a quarter of the power of the fall.

Let us consider a bird gliding with a uniform movement, the plane of its wings AB being inclined at the small angle α, below the horizon AH. It will descend along the line AM, inferior to AB and forming with it the angle $BAM = \beta$.



Then let P be the weight of the bird; V its velocity; S the lower surface of its wings; c sine 1° the normal resistance that a surface similar to S, but 1 mq would experience when moving at an angle of 1° and a speed of 1 metre per second; S′ the projection of the bird upon a plane perpendicular to the plane of its wings and the axis of its body; $c'S'$ the resistance that the bird experiences in advancing along this axis with a velocity of 1 metre; T the work given out during the fall in one second, being the sum of the work T_1 and T_2 of suspension and propulsion.

As the bird is sustained by its surface S, the vertical component of the resistance of the air upon S balances the weight P: we thus have $cS\ V^2$ sine β cos. $\alpha = P$; but as α is small, cos. α is sensibly equal to one, and we can thus write simply $cS\ V^2$ sine $\beta = P$. (1)

The work of suspension during one second is

$$T_1 - cS\ V^3 \text{ sine}^2\ \beta.$$

Substituting for V in this equation its value got from (1), we have

$$T_1 = P \sqrt{\frac{P}{cS}} \sqrt{\text{sine } \beta}.$$

This remarkable conclusion shows us, that in the uniform movement of a plane gliding in the air, the work necessary for its suspension diminishes at least proportionately to the square root of the sine of the angle with which the air is attacked. The work necessary thus tends towards zero when the velocity is increased indefinitely.

But the work of progression, which is nothing if $\beta = 90°$ increases as β, is less. The total work $T = T_1 + T_2$ has thus a minimum corresponding to a certain value of V, and consequently of α and β.

But taking into account the smallness of $c'S'$ in comparison with cS, in all birds and bats, it is seen at first sight that β will be small, as will also the value of α, which is necessary for the maintenance of the impulse. (This will be verified further on.)

We shall now have $T_2 = c'S'\ V^3$,

And $T = T_1 + T_2 = cS\ V^3 \text{ sine}^2\ \beta + c'S'\ V^3$. (2)

Let us find the minimum of this quantity. From (1) we get $\text{sine}^2\ \beta = \dfrac{P^2}{c^2\ S^2\ V^4}$

Substituting it in (2) we have $T = \dfrac{P^2}{cS\ V} + c'S'\ V^3$.

As an equivalent to O, the derivative of the second member in comparison with V, we have the value of V corresponding to the minimum of T.

$$3\,c'S'\,V^2 - \frac{P^2}{cS\,V^2} = 0.$$

$$\text{From whence } V^4 = \frac{P^2}{3\,cS\,c'S'} \qquad (3)$$

(The second derivative is besides positive: this value corresponds well with a minimum.)

We then have $\text{sine}^2\,\mathfrak{C} = 3\,\frac{c'S'}{cS}$

α, the resistance to the forward motion, being evidently equal to the compounds of the weight parallel to the plane of the wings, we get $c'S'\,V^2 = P \text{ sine } \alpha$, from which we deduce

$$\text{sine}^2\,\alpha = \frac{1\;c'S'}{3\;cS} \qquad \text{sine } \mathfrak{C} = 3 \text{ sine } \alpha.$$

The work T becomes

$$T = \frac{4}{3}\sqrt[4]{3}\,P\sqrt{\frac{P}{cS}}\sqrt[4]{\frac{c'S'}{cS}}$$

Lastly, the comparison of the work of suspension to the work of propulsion is, as we have stated, found independent of data $\frac{T_1}{T_2} = 3.$

Note—If in place of using the law of the simple sine of the angle of incidence we employ the sine^n, we have

$$\frac{T_1}{T_2} = \frac{\text{sine } \mathfrak{C}}{\text{sine } \alpha} = \frac{3\,n}{2 - n}$$

which shows that, according to the old law of sine^2, the most economical planing would be a vertical fall.

THEOREM II.

A bird moving at a uniform velocity travels, in gliding a given distance, with the least fall possible, when the work of

suspension is sensibly equal to the work of translation. The plane of the wings then divides into two equal parts the angle formed with the horizon by the path of the bird, and this angle is itself as small as possible.

We see from this that the path followed by the bird is nearly horizontal, for to say that the fall is at a minimum in travelling a given distance is equivalent to saying that, for a given fall, the bird travels the greatest distance, and consequently in the nearest possible path to the horizon.

Retaining our notation, except that T T_1 T_2 do not now refer to a second of time, but to the total time necessary to travel the given distance which we will call e, we have

$$e = \mathrm{V}\, t \qquad (1)$$

$$c\mathrm{S}\, \mathrm{V}^2 \text{ sine } \varepsilon = \mathrm{P} \qquad (2)$$

$$T = T_1 + T_2 = c\mathrm{S}\, \mathrm{V}^3 \text{ sine}^2\, \varepsilon\, t + c'\mathrm{S}'\, \mathrm{V}^3\, t \qquad (3)$$

$$\text{From (2) we get } \mathrm{V}^2 = \frac{\mathrm{P}}{c\mathrm{S} \text{ sine } \varepsilon}$$

$$\text{Multiplied by (1) it becomes } \mathrm{V}^3\, t = \frac{e\mathrm{P}}{c\mathrm{S} \text{ sine } \varepsilon}$$

Substituting it in (3) we have $T = e\mathrm{P} \text{ sine } \varepsilon + \dfrac{c'\mathrm{S}'\, e\mathrm{P}}{c\mathrm{S} \text{ sine } \varepsilon}$

It is this quantity that must be reduced to its minimum. This will be when $T_1 = T_2$, since the product $T_1\, T_2$ is constant.

$$T_1 = T_2$$

$$\text{We find then sine}^2\, \varepsilon = \frac{c'\mathrm{S}'}{c\mathrm{S}} = \text{sine}^2\, \alpha$$

$$\text{We have also } \mathrm{V}^4 = \frac{\mathrm{P}^2}{c\mathrm{S}\, c'\mathrm{S}'}$$

$$\text{And the work per second } \frac{T}{t} = 2\mathrm{P} \sqrt{\frac{\mathrm{P}}{c\mathrm{S}}} \sqrt{\frac{c\mathrm{S}'}{c\mathrm{S}}}$$

from which we learn that the work is here $\frac{8}{7}$ of that given by the former theorem, while the speed is equal to $\frac{4}{5}$.

If the direction of flight is against the wind it will give

the advantage of augmenting the speed. In the case of a favourable wind it would, on the contrary, be similar to the conditions of the preceding theorem.

Note—The law of sine^n gives $\frac{T_1}{T_2} = \frac{\text{sine } \epsilon}{\text{sine } \alpha} = n$. and for the law of sine^2 $T_1 = 2T_2$. We see, then, that even in this hypothesis the translation requires still a third part of the total work.

III.

In these calculations it is taken for granted that the resistance to the forward motion is small compared to the resistance of the descent, or. to be more exact. $\frac{c' S'}{c S}$ is small. which is always the case in flying beings. excepting a few insects.

It is the same in the case of a surface that is inclined above the horizon instead of below it, and being moved in a nearly horizontal line instead of falling under its own weight. We are thus able to get valuable information, not only concerning the flight of birds, but upon the value of a greater part of the proposed systems of aviation.

We shall now notice the most general of the numerous consequences of these calculations.

1st—Every one knows that the ordinary flight and the gliding of the same bird are always in concordance with regard to speed, maintenance. &c.: that it can immediately pass from one to the other with great facility. which fact proves their near relationship. In fact the bird in ordinary flight moves at an almost uniform velocity. and may be considered as a plane gliding at small but variable inclinations. and it has even (so much does the economy of ordinary flight permit) the advantage of attacking the air almost at the same angle over its whole surface. It is thus certain that the preceding

calculations are applicable to ordinary flight, at least as to the form of connection established between the different elements of the problem, the co-efficients alone being slightly changed. All that follows applies to every kind of forward flight.

2nd—$c'S'$, cS, and P differ but little in the same species of flying beings, but vary in different species, and thus the necessity of determining for each specie its own proper velocity. This velocity will be greater as cS and $c'S'$ are less and P greater, in other words that the bird will be heavier, possess less sustaining surface, and be a better projectile.

Thus we explain the fact which has astonished many naturalists, that a bird with wings relatively small generally flys better than those with large wings.

We also understand the reason why birds reduce the area of their wings and shut their tails when they wish to fly quickly, for instance the ringdove. It is owing to the power of varying the area of their wings that birds possess such suppleness of flight. Insects and bats not possessing this power have an eccentricity in their movements similar to those made by a sheet of paper abandoned to itself in the air.

3rd—The work expended per second in order to sustain a given weight is proportionately less as cS is greater relatively to the weight.

This rule shows that a bird has more labour to sustain itself the larger it is, for the amount of surface per kilogramme is as the inverse of the size. This has been thought by many the rock upon which aviation would be wrecked. It is necessary, however, to remember that the flight becomes more rapid as the bird is larger, so much so that if the work per kilogramme and per second increases with the size the work per kilogramme and for a given distance remains independent of the weight P of the bird, the largest bird therefore being able to make at least as long a flight as the small ones.

From another point of view this difficulty of suspension in machines capable of carrying men, which we must boldly face, must not be so much deplored, for if we possess the same facilities of suspension as the small birds we should also have their restricted speed: but the special aim and necessity of aërial navigation and, above all, aviation, is speed, and it is easier to obtain it in a large machine than a small one.

4th—The work expended depends a great deal upon $c'S'$, so that when the resistance of the machine to the forward motion is reduced, not only is the speed augmented but the work necessary for its suspension in the air is also reduced. It is for this reason that birds are formed and feathered in a fashion to make good projectiles, especially the good flyers, and also why they draw their legs up under their tails and stretch out their necks. If the heron does not do so it is because its body would still remain so ill-shaped, and the reduction in the resistance would not compensate for the fatigue. It does do it, however, when pursued by a bird of prey.

If large wings indicate easy flight (for it is evident that nature has not made them without a cause) small ones do not necessarily show small power of flight, if the pectoral muscles are powerful, or even if the body is a good projectile; for instance king-fishers and ducks.

All birds with angular bodies, and which have to fly considerable distances in order to find subsistence, are furnished with ample wings; the gralla for instance.

On the other hand the divers with spindle-shaped bodies are able to effect their migrations with a minimum of wing surface.

A great development of the parachute surface, especially when the weight is small, increases considerably the resistance to the forward motion, so that the advantage is not so great as at first might be thought.

It is doubtless owing to their inferiority as projectiles that the size of bats, and especially insects, never equals that of birds.

5th—The angle at which the air is attacked (β) and the angle of favourable fall ($\alpha + \beta$), whose ratio is fixed, are independent of the weight, increasing with $c'S'$ and diminishing with cS. These angles can be accurately obtained for each specie, as it only depends upon $\frac{c'S'}{cS}$, which evidently varies little between individuals, whilst P, especially in solitary birds, shows considerable variations.

6th—For similar flying beings the velocity and the relative work are proportionate to the square root of the homologous dimensions, and in this case the number of beats of the wings, supposing them to be of equal amplitude, is inversely proportionate to the square root of the dimensions, which fact has been pointed out by M. Hureau de Villeneuve. The angles of flight, that is to say the manner of flight, remain undisturbed.

7th—All our calculations and their results are applicable to vertical screws whilst hovering, but not when advancing laterally, for then the movements of the wings relatively to the air is not uniform. It must be understood that every time the machine ascends and descends, the force of gravity, useful or injurious, modifies proportionately the work of the motive power.

8th—The calculations can be applied to all varieties of inclined planes, provided with propellers like my model aëro-planes, but if we take into account the efficacy of the propeller, it will be of advantage to attack the air still more obliquely, for the thrust of the propeller becoming less its slip will be reduced.

As the useful work or efficient thrust of the propeller is divided in a determined ratio between the suspension and the

forward motion, we see that the thrust is a given multiplicator. and besides small compared to that necessary for the machine to cleave the air. We are then led to the following remarkable theorem.

The area of the propellers must always be proportioned to resistance of the forward motion $c'S'$ and not to the weight of the machine, which is also without direct influence upon the size of the rudders and other auxiliary surfaces. The area is also independent of the density of the fluid in which the machine moves and its velocity, which is evident in the propulsion of ships.

Before discovering these laws in 1870 I was puzzled to know how the flying-fish was able to sustain itself with its small pectoral muscles and wings badly formed for beating the air. These calculations led me to think that the fish (whilst at the same time taking advantage, as the sea-birds do, of the current of air ascending along the slopes of the waves) propels itself not with its wings but its tail. Since then I have had the pleasure of knowing that these views are entirely confirmed by the observations of MM. de Tessan and Agassiz.

9th—If the work of translation exceeds half the total work it increases at first very slowly, and thus aviation, which allows of compact and spindle-shaped forms, is veritably for moderate masses the most economical mode of transport for high velocities, since it is only necessary to cleave the air.

Note—Several of these results, and amongst them those relating to the propellers, subsist with another law of the resistance of the air.

IV.

At length our calculations, combined with certain results of observation, allow us to obtain very nearly the co-efficient c and c' of resistance to flying bodies, and the angle at which they attack the air when they glide.

It is also, as we have remarked, true enough of ordinary flight.

When the raven, for example, is about to alight upon the ground he ceases to flap his wings and glides with regular movements till near the ground, which he follows for some metres whilst he retards his velocity.

I have watched them alight thus in a place surrounded by high poplars, and noted the time and height when at the beginning of its descent, and also the place and time where it touched the ground.

It was only necessary to measure the distance between the tree and this spot, the height of the branch from which the bird dropped, in order to obtain the velocity V and the work expended per second.

I have made four or five such experiments in calm weather, which is absolutely necessary if we wish to obtain accurate results, and I here take the mean of two observations in which the flight was most horizontal.

It is admitted by theorem II that in this case the work was equally divided between the translation and the suspension, for the bird ceases to flap his wings from motives of economy, and thus should cease them as soon as possible.

I have found, having regard to the height due to the initial velocity, that the raven descends 4ft. 5in. per second with a velocity of 36ft. 1in. This agrees very well with the observations of the late Sir George Cayley.

The mean of a large number of measurements, in the case of the raven, gives for the weight P = 505gr. (about 1·11lbs.), 44c. (about $17\frac{1}{3}$in.), as the length of wing 19·4c. ($7\frac{2}{3}$in.), as the mean width 17c. ($6\frac{3}{4}$in.), as the length of tail (from the roots of the feathers), and 38c. (15in.) as the length of the body (the beak to the commencement of the tail). This last number, along with the circumference of the body, 28c. (11in.),

gives the projectile form of the bird. The mean thickness of the wing is about 9mm. (about $\frac{1}{3}$in.)

From the above figures the amount of surface exposed by the bird in the case under consideration would be about S = 0mq., 185, 1500cq. (about $232\frac{1}{2}$ sq. in.), and 350cq. (about 54in.) for the body and tail.

The inferior surface of the body, owing to its rounded form, offers little resistance where the air is not retained laterally by the wings. The tail will only sustain to a moderate extent, for it is less inclined to the line of flight than the wings are, as I have shown to be necessary in *l'Aëronaute* for January, 1872.

With regard to S′ the trunk of the bird is 60cq. ($9\frac{1}{3}$ sq. in.). and the edge of the wings 80cq. ($12\frac{1}{2}$ sq. in.), which is a fact that few persons know I believe.

We thus have S′ = 0mq. ·014, and can now find the unknown factors, first observing that the ratio of the fall to the velocity is exactly sine $(\alpha + \zeta) = \text{sine } 2\,\zeta = \frac{1^{m}\,35}{11^{m}} = \text{sine } 7^{\circ}$ Whence $\alpha = \zeta = 3^{\circ}\ 30$. Now the equation $c\,S\,V^2 \text{ sine } \zeta = P$ gives $c = \frac{P}{S\ V^2 \text{ sine } \zeta}$, and the resistance of a surface 1mq., in moving at a velocity of 1m. at an angle of 10° will be $c \text{ sine } 10^{\circ} = \frac{P \text{ sine } 10^{\circ}}{S\ V^2 \text{ sine } \zeta} =$ 64gr., which is a third more than had been found by Thibault, which shows the advantage of using a convenient form and curve. The sea-gull, of which the wings are narrow, gives, by the same method, a slightly superior result.

Again from $c' = \frac{P \text{ sine } \alpha}{S'\ V^2} =$ 18·5 gr., the raven, thanks to its form, cleaves the air seven times more easily than a flat surface of the same section. This agrees very well with what

we know of the resistance to ships, which in cases where there is an analagous relation between the rubbing surface and the cross section, the friction is, roughly speaking, about half the resistance. Applying this rule to our raven we have a perception of the co-efficient of friction F, the surface of the bird, which is slightly superior to 2 S. We find for the friction of a surface of 1mq., moving at a speed of 1m., F = 0·35gr. nearly.

This value is plausible.

As all these results are drawn from a few observations difficult to make, we must not regard them as absolute; but I believe them to be so. I believe them, with the exception of F, to be exact to within a fifth.

It would be very interesting to collect a large number of similar data of different flying beings, especially large birds, so that we could get several means.

If we do not accept the law of the simple sine of the angle of incidence, we are obliged to renounce that of sine^2 generally recognised, since it gives from known tables a fall of 13ft. 1in. per second to the raven, even when we do not take into account the translation, whilst, as we have just seen, the real fall is only 4ft. 5in. The advantage of the oblique is manifest.

It is thus that the flight of birds, incessantly proving the resistance of the air, can give us valuable data, and if it does not replace the result of special experiments, at least it will guide and light us considerably in the difficult task.

It has been thought judicious to include in these Annual Reports, as far as space will allow, all the Literature upon the subject which is worth re-producing. In this category we place the late Sir George Cayley's recorded experience.

ON AËRIAL NAVIGATION,

BY

SIR GEORGE CAYLEY, BART.

Reprinted from "Nicholson's Journal" for 1809 *&* 1810.

Since the days of Bishop Wilkins the scheme of flying by artificial wings has been much ridiculed, and indeed the idea of attaching wings to the arms of a man is ridiculous enough, as the pectoral muscles of a bird occupy more than two-thirds of its whole muscular strength, whereas in man the muscles that could operate upon the wings thus attached would probably not exceed one-tenth of the whole mass. There is no proof that, weight for weight, a man is comparatively weaker than a bird; it is therefore probable, if he can be made to exert his whole strength advantageously upon a light surface similarly proportioned to his weight, as that of the wing to the bird, that he would fly like a bird. The flight of a strong man by great muscular exertion, though a curious and interesting circumstance, inasmuch as it will probably be the first means of ascertaining this power and supplying the basis whereon to improve, it would be of little use. I feel perfectly confident, however, that this noble art will soon be brought home to man's general convenience, and that we shall be able to transport ourselves and families, and their goods and chattels, more securely by air than by water, and with a velocity of from 20 to 100 miles per hour. To produce this effect it is only necessary to have a first mover, which will generate more power

in a given time, in proportion to its weight, than the animal system of muscles.

The consumption of coal in a Boulton & Watt's steam engine is only about $5\frac{1}{2}$lbs. per hour for the power of one horse. The heat produced by the combustion of this portion of inflammable matter is the sole cause of the power generated, but it is applied through the intervention of a weight of water expanded into steam, and a still greater weight of cold water to condense it again. The engine itself likewise must be massive enough to resist the whole external pressure of the atmosphere, and therefore is not applicable to the purpose proposed. Steam engines have lately been made to operate by expansion only, and these might be constructed so as to be light enough for this purpose, provided the usual plan of a large boiler be given up and the principle of injecting a proper charge of water into a mass of tubes, forming the cavity for the fire, be adopted in lieu of it. The strength of vessels to resist internal pressure being inversely as their diameters, very slight metallic tubes would be abundantly strong, whereas a large boiler must be of great substance to resist a strong pressure. The following estimate will show the probable weight of such an engine with its charge for one hour:—

	lbs.
The engine itself	90 to 100
Weight of inflamed cinders in a cavity presenting about 4ft. surface of tube ...	25
Supply of coal for one hour	6
Water for ditto, allowing steam of one atmosphere to be $\frac{1}{1800}$, the specific gravity of water	32
	163

I do not propose this statement in any other light than

as a rude approximation to truth, for as the steam is operating under the disadvantage of atmospheric pressure it must be raised to a higher temperature than in Messrs. Boulton & Watt's engine, and this will require more fuel; but if it take twice as much still the engine would be sufficiently light, for it would be exerting a force equal to raising 550lbs. one foot high per second, which is equivalent to the labour of six men, whereas the whole weight does not much exceed that of a man.

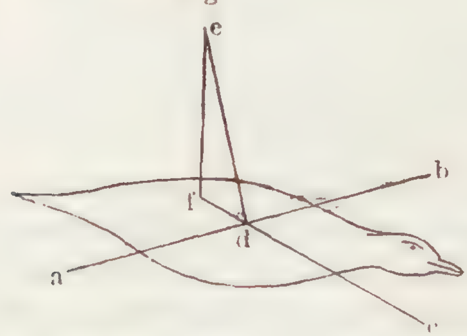
It may seem superfluous to enquire further relative to a first mover for aërial navigation, but lightness is of so much value in this instance that it is proper to notice the probability that exists of using the expansion of air, by the sudden combustion of inflammable powders or fluids, with great advantage. The French have lately shown the great power produced by igniting inflammable powders in close vessels, and several years ago an engine was made to work in this country in a similar manner by inflammation of spirit of tar. I am not acquainted with the name of the person who invented this engine, but from some minutes with which I was favoured by Mr. William Chapman, of Newcastle, I find that 30 drops of oil of tar raised 8cwt. to the height of 22in.; hence 1 horse-power would consume from 10 to 12lbs. per hour, and the engine itself need not exceed 50lbs. weight. I am informed by Mr. Chapman that this engine was exhibited in a working state to Mr. Rennie, Mr. Cartwright, and several other gentlemen capable of appreciating its powers, but that it was given up in consequence of the expense attending its consumption being about eight times greater than that of a steam engine of the same power. Probably a much cheaper engine of this sort might be produced by a gas-light apparatus and by firing the inflammable air generated with a due portion of common air under a piston. Upon some of these principles it is perfectly clear that force can be obtained by a much lighter apparatus than the muscles

of animals or birds, and therefore in such proportion may aërial vehicles be loaded with inactive matter. Even the expansion steam engine, doing the work of six men and only weighing equal to one, will as readily raise five men into the air as one man can elevate himself by his own exertions, but by increasing the magnitude of the engine 10, 50, or 500 men may be equally well conveyed, and convenience alone, regulated by the strength and size of materials, will point out the limit for the size of vessels in aërial navigation.

Having rendered the accomplishment of this object probable upon the general view of the subject, I shall proceed to point out the principles of the art itself. For the sake of perspicuity I shall, in the first instance, analyze the most simple action of the wing in birds, although it necessarily supposes many previous steps.

When large birds, that have a considerable extent of wing compared with their weight, have acquired their full velocity, it may frequently be observed that they extend their wings, and without waving them continue to skim for some time in a

Fig. 1.



horizontal path. Fig. 1 represents a bird in this act. Let *ab* be a section of the plane of both wings opposing the horizontal current of air (created by its own motion), which may be represented by the line *cd*, and is the measure of the velocity

of the bird. The angle *bdc* can be increased at the will of the bird, and to preserve a perfectly horizontal path, without the wing being waved, must continually be increased in a complete ratio (useless at present to enter into) till the motion is stopped altogether; but at one given time the position of the wings may be truly represented by the angle *bdc*. Draw *de* perpendicular to the plane of the wings, produce the line *cd* as far as required, and from the point *e*, assumed at pleasure in the line *de*, let fall *ef* perpendicular to *df*; then *de* will represent the whole force of the air under the wing, which being resolved into the two forces *ef* and *fd* the former represents the force that sustains the weight of the bird, the latter the retarding force by which the velocity of the motion producing the current *cd* will be continually diminished; *ef* is always a known quantity, being equal to the weight of the bird, and hence *fd* is also known as it will always bear the same proportion to the weight of the bird as the sine of the angle *bdc* bears to its cosine, the angles *def* and *bdc* being equal. In addition to the retarding force thus received is the direct resistance which the bulk of the bird opposes to the current. This is a matter to be entered into separately from the principles now under consideration, and for the present may be wholly neglected under the supposition of its being balanced by a force precisely equal and opposite to itself.

Before it is possible to apply this basis of the principle of flying in birds to the purpose of aërial navigation it will be necessary to encumber it with a few practical observations.

The whole problem is confined within these limits, viz.—To make a surface support a given weight by the application of power to the resistance of air. Magnitude is the first question respecting the surface. Many experiments have been made upon the direct resistance of air by Mr. Robins, Mr. Rouse, Mr. Edgeworth, Mr. Smeaton, and others. The result of

Mr. Smeaton's experiments and observations was that a surface of a square foot met with a resistance of 1lb. when it travelled perpendicularly to itself through air at a velocity of 21ft. per second. I have tried many experiments upon a large scale to ascertain this point. The instrument was similar to that used by Mr. Robins, but the surface used was larger, being an exact square foot, moving round upon an arm about 5ft. long, and turned by weights over a pulley. The time was measured by a stop-watch, and the distance travelled over in each experiment was 600ft. I shall only give the results of many carefully-repeated experiments, which are, that a velocity of 11·538ft. per second generated a resistance of 4oz., and that a velocity of 17·16ft. per second gave 8oz. resistance. This delicate instrument would have been strained by the additional weight necessary to have tried the velocity generating a pressure of 1lb. per square foot; but if the resistance be taken to vary as the square of velocity, the former will give the velocity necessary for this purpose at 23·1ft., the latter 24·28ft. per second. I shall therefore take 23·6ft. as somewhat approaching the truth.

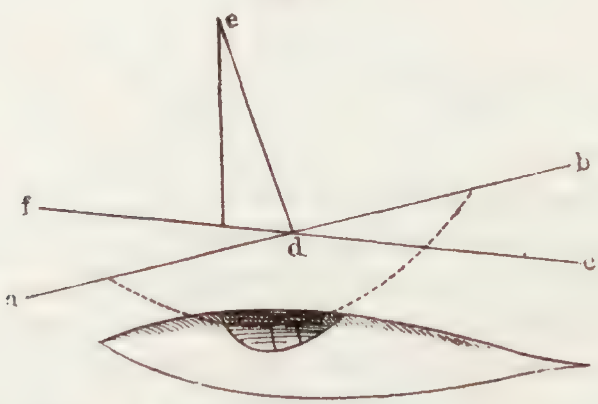
Having ascertained this point, had our tables of angular resistance been complete, the size of the surface necessary for any given weight would easily have been determined. Theory. which gives the resistance of a surface opposed to the same current in different angles, to be as the square of the sine of the angle of incidence, is of no use in this case, as it appears, from the experiments of the French Academy, that in acute angles the resistance varies much more nearly in the direct ratio of the sines than as the squares of the sines of the angle of incidence. The flight of birds will prove to an attentive observer that, with a concave wing apparently parallel to the horizontal path of the bird, the same support and, of course. resistance is obtained; and hence I am inclined to suspect that

under extremely acute angles, with concave surfaces, the resistance is nearly similar in them all. I conceive tho operation may be of a different nature from what takes place in larger angles, and may partake more of the principle of pressure exhibited in the instrument known by the name of the hydrostatic paradox. A slender filament of the current is constantly received under the anterior edge of the surface and directed upward into the cavity by the filament above it being obliged to mount along the convexity of the surface, having created a slight vacuity immediately behind the point of separation. The fluid accumulated thus within the cavity has to make its escape at the posterior edge of the surface where it is directed considerably downward, and therefore has to overcome and displace a portion of the direct current passing with its full velocity immediately below it; hence whatever elasticity this effort requires operates upon the whole concavity of the surface. excepting a small portion of the anterior edge. This may or may not be the true theory, but it appears to me to be the most probable account of a phenomenon which the flight of birds proves to exist.

Six degrees was the most acute angle, the resistance of which was determined by the valuable experiments of the French Academy, and it gave $\frac{4}{10}$ of the resistance which the same surface would have received from the same current when perpendicular to itself. Hence, then, a superficial foot, forming an angle of six degrees with the horizon, would, if carried forward horizontally (as a bird in the act of skimming) with a velocity of 23·6ft. per second, receive a pressure of $\frac{4}{10}$ of a pound perpendicular to itself; and if we allow the resistance to increase as the square of the velocity at 27·3ft. per second, it would receive a pressure of 1lb. I have weighed and measured the surface of a great many birds, but at present shall select the common rook. because its surface and weight are as nearly

as possible in the ratio of a superficial foot to a pound The flight of this bird, during any part of which they can skim at pleasure, is (from an average of many observations) about 34·5ft. per second. The concavity of the wing may account for the greater resistance here received than the experiments upon plane surfaces would indicate. I am convinced that the angle made use of in the crow's wing is much more acute than 6 degrees; but in the observations that will be grounded upon these data I may safely state that every foot of such curved surface, as will be used in aërial navigation, will receive a resistance of 1lb. perpendicular to itself when carried through the air in an angle of 6 degrees with the line of its path at a velocity of about 34 to 35ft. per second.

Fig. 2.



Let *ab*, Fig. 2, represent such a surface or sail made of thin cloth, and containing about 200 square feet (if of a square form the side will be a little more than 14ft.), and the whole of a firm texture. Let the weight of the man and the machine be 200lbs. Then if a current of wind blew in the direction *cd* with a velocity of 35ft. per second, at the same time that a cord. represented by *cd*, would sustain a tension of 21lbs.. the machine would be suspended in the air. or at least be within a few ounces

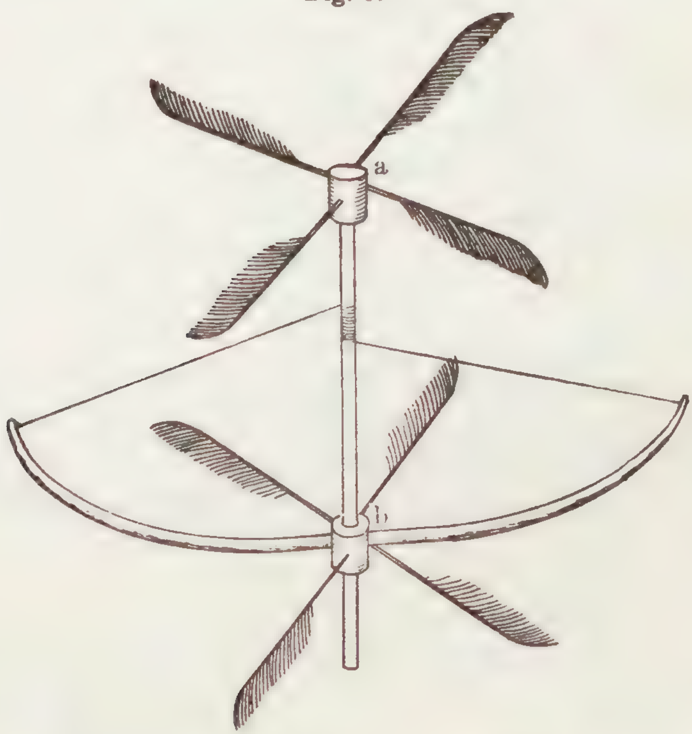
of it (falling short of such support cnly in the ratio of the sine of the angle of 94 degrees compared with the radius, to balance which defect suppose a little ballast to be thrown out), for the line *de* represents a force of 200lbs., which, as before being resolved into *df* and *fe*, the former will represent the resistance in the direction of the current, and the latter that which sustains the weight of the machine. It is perfectly indifferent whether the wind blow against the plane or the plane be driven with an equal velocity against the air. Hence if this machine were pulled along by a cord, *cd*, with a tension of about 21lbs., at a velocity of 35ft. per second, it would be suspended in a horizontal path; and if, in lieu of this cord, any other propelling power were generated in this direction, with a like intensity, a similar effect would be produced. If therefore the waft of surfaces advantageously moved by any force generated within the machine took place to the extent required, aërial navigation would be accomplished. As the acuteness of the angle between the plane and current increases, the propelling power required is less and less. The principle is similar to that of the inclined plane, in which, theoretically, 1lb. may be made to sustain all but an infinite quantity, for in this case if the magnitude of the surfaces be increased *ad infinitum*, the angle with the current may be diminished, and consequently the propelling force in the same ratio. In practice the extra resistance of the car and other parts of the machine, which consume a considerable portion of power, will regulate the limits to which this principle, which is the true basis of aërial navigation, can be carried, and the perfect ease with which some birds are suspended in long horizontal flights, without one waft of their wings. encourages the idea that a slight power only is required.

I have myself made a large machine on this principle, large enough for aërial navigation, but which I have not had

an opportunity to try the effect of, excepting as to its proper balance and security. It was beautiful to see this noble white bird, sail majestically from the top of a hill to any given point of the plane below it with perfect steadiness and safety, according to the set of its rudder, merely by its own weight descending in an angle of about 8 degrees with the horizon.

As it may be amusing to some of my readers to see a machine rise in the air by mechanical means, the following is a description of one of which any one can construct at the

Fig. 3.



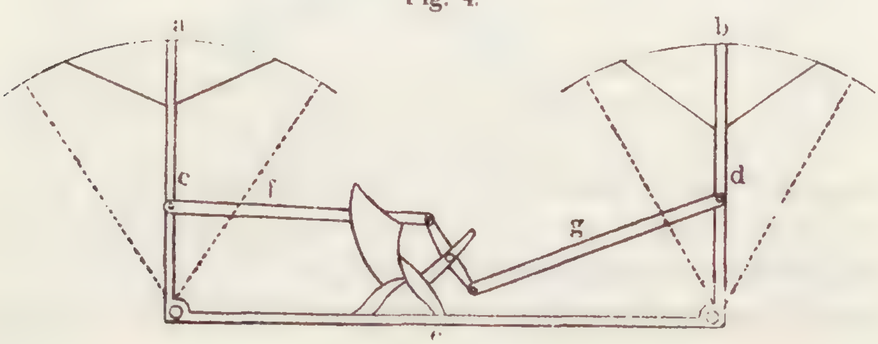
expense of ten minutes' labour:—*a* and *b*. Fig. 3, are two corks, into each of which are inserted four wing feathers, from any bird, so as to be slightly inclined like the sails of a windmill, but in opposite directions in each set. A round shaft is fixed

in the cork *a*, which ends in a sharp point. At the upper part of the cork *b* is fixed a whalebone bow, having a small pivot hole in its centre to receive the point of the shaft. The bow is then to be strung equally on each side to the upper portion of the shaft, and the little machine is completed. Wind up the string by turning the flyers different ways, so that the spring of the bow may unwind them with their anterior edges ascending. Then place the cork with the bow attached to it upon a table, and, with the finger on the upper cork press strong enough to prevent the string unwinding and taking it away suddenly, the machine will rise to the ceiling. This was the first experiment I made upon this subject in the year 1796.

If in lieu of these small feathers large planes, containing together 200 square feet, were similarly placed, or in any other more convenient position, and were turned by a man or first mover of adequate power, a similar effect would be the consequence, and for the mere purpose of ascent this is perhaps the best apparatus: but speed is the great object of this invention, and this requires a different structure.

In lieu of applying the continued action of the inclined plane, by means of the rotative motion of flyers, the same principle may be made use of by the alternative motion of surfaces backward and forward, as in the following manner:—

Fig. 4.



Let *a* and *b*, Fig. 4, be two surfaces or parachutes supported upon the long shafts *c* and *d*, which are fixed to the ends of the connecting beam *e* by hinges. At *e* let there be a convenient seat for the aëronaut, and before him a cross-bar turning upon a pivot in the centre, which, being connected with the shafts of the parachute by the rods *f* and *g*, will enable him to work them alternately backwards and forwards, as represented by the dotted lines. If the upright shaft be elastic or have a hinge to give way a little, near their tops, the weight and resistance of the parachute will incline them so as to make a small angle with the direction of their motion, and hence the machine rises. A slight heeling of the parachute towards one side, or an alteration in the position of the weight, may enable the aëronaut to steer such an apparatus tolerably well; but many better constructions may be formed for combining the requisites of speed, convenience, and steerage.

Having described the general principle of support in aërial navigation, I shall proceed to show how this principle must be applied so as to be steady and manageable. Several persons have ventured to descend from balloons in a parachute which exactly resembles a large umbrella, with a light car suspended by cords underneath it. It is very remarkable that the only machines of this sort which have been constructed are nearly of the worst possibly form for producing a steady descent—the purpose for which they are intended. To render this subject more familiar let us recollect that in a boat swimming upon water its stability or stiffness depends, in general terms, upon the weight and distance from the centre of the section elevated above the water, by any given heel of the boat on one side; and on the bulk and its distance from the centre, which is immersed below the water on the other side, the combined endeavour of the one to fall and the other to swim produces the desired effect in a well-constructed boat.

The centre of gravity of the boat being more or less below the centre of suspension is an additional cause of its stability.

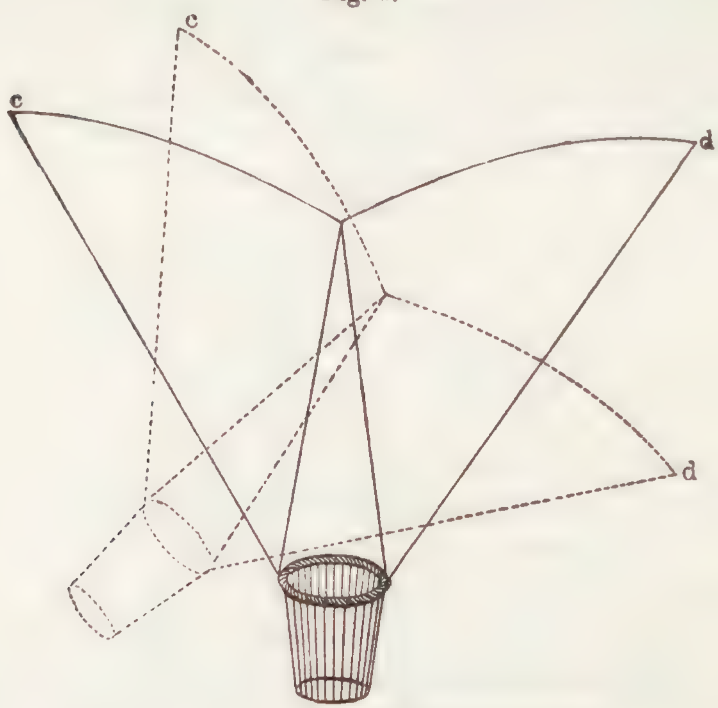
Let us now examine the effect of a parachute represented

Fig. 5.

by *ab*, Fig. 5. When it has heeled into the position represented by the dotted lines, *a* is become perpendicular to the current created by the descent, and therefore resists with its greatest power; whereas the side *b* is become more oblique, and of course its resistance is much diminished. Hence, so far as this form of the sail or plane is regarded, it operates directly in opposition to the principle of stability, for the side that is required to fall resists much more in its new position, and that which is required to rise resists much less; therefore complete inversion would be the consequence if it were not for the weight being suspended so very much below the surface, which, counteracting this tendency, converts the effort into a violent oscillation.

On the contrary, let the surface be applied in the inverted

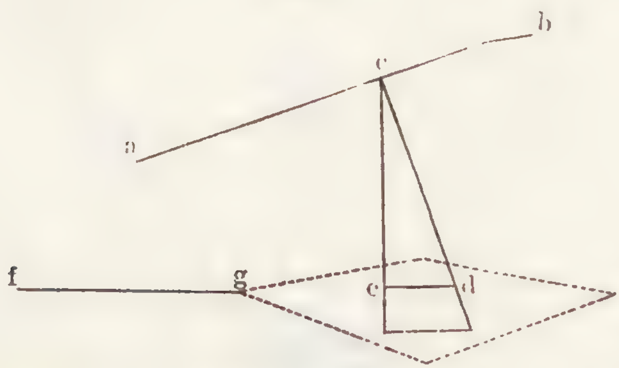
Fig. 6.



position as represented at *cd*, Fig. 6, and suppose it to be heeled to the same angle as before represented by the dotted lines *cd*. Here the exact inverse of the former instance takes place, for that side which is required to rise has gained resistance by its new position, and that which is required to sink has lost it; so that as much power operates to restore the equilibrium in this case as tended to destroy it in the other, the operation very much resembling what takes place in the common boat. This angular form, with apex downwards, is the chief basis of stability in aërial navigation; but as the sheet which is to suspend the weight attached to it in its horizontal path through the air must present a slightly concave surface in a small angle with the current, this principle can only be used in the lateral

extension of the sheet. and this most effectually prevents any rolling of the machine from side to side. Hence the section of the inverted parachute, Fig. 6, may equally well represent the cross section of a sheet for aërial navigation. The principle of stability in the direction of the path of the machine must be derived from a different source.

Fig. 7.



Let *ab*, Fig. 7, be a longitudinal section of a sail, and let *c* be its centre of resistance, which experiment shows to be considerably more forward than the centre of the sail. Let *cd* be drawn perpendicular to *ab*, and let the centre of gravity of the machine be at any point in that line as at *d*; then if it be projected in a horizontal path. with velocity enough to support the weight. the machine will retain its relative position like a bird in the act of skimming, for drawing *ce* perpendicular to the horizon, and *de* parallel to it, the line *ce* will, at some particular moment, represent the supporting power and likewise its opponent. the weight; and the line *de* will represent the retarding power and its equivalent, that portion of the projectile force expended in overcoming it; hence, these various powers being exactly balanced. there is no tendency in the machine but to proceed in its path with its remaining portion of projectile force.

The stability in this position, arising from the centre of gravity being below the point of suspension, is aided by a remarkable circumstance that experiment alone could point out. In very acute angles with the current it appears that the centre of resistance in a sail does not coincide with the centre of its surface, but is considerably in front of it. As the obliquity of the current decreases these centres approach and coincide when the current becomes perpendicular to the sail. Hence any heel of the machine backward or forward removes the centre of support behind or before the point of suspension, and operates to restore the original position by a power equal to the whole weight of the machine, acting upon a lever equal in length to the distance the centre has removed.

To render the machine perfectly steady, and likewise to enable it to ascend and descend in its path, it becomes necessary to add a rudder in a similar position to the tail in the bird. Let *fg* be the section of such a surface parallel to the current and let it be capable of moving up and down upon *g* as a centre, and of being fixed in any position. The powers of the machine being previously balanced, if the least pressure be exerted by the current either upon the upper or under surface of the rudder, according to the will of the aëronaut, it will cause the machine to rise or fall in its path so long as the propelling force is continued with sufficient energy.

From a variety of experiments upon this subject I find that when the machine is going forward, with a superabundant velocity, or that which would induce it to rise in its path, a very steady horizontal course is effected by a considerable depression of the rudder, which has the advantage of making use of this portion of sail in aiding the support of the weight. When the velocity is becoming less, as in the act of alighting, then the rudder must gradually recede from this position and even become elevated for the purpose of preventing the machine

from sinking too much in front, owing to the combined effect of the want of projectile force sufficient to sustain the centre of gravity in its usual position, and of the centre of support approaching the centre of the sail.

The elevation and depression of the machine are not the only purposes for which the rudder is designed. This appendage must be furnished with a vertical sail and be capable of turning from side to side in addition to its other movements, which effects the complete steerage of the vessel.

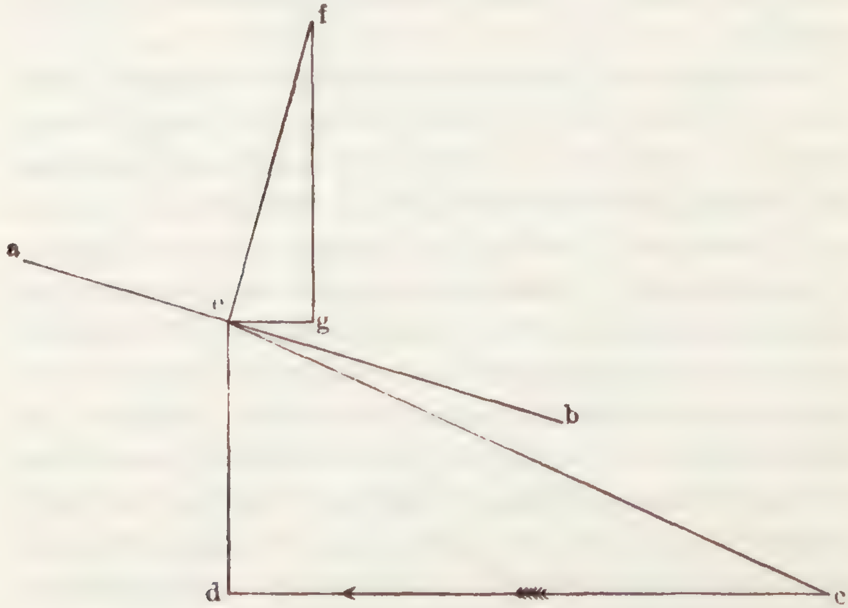
All these principles upon which the support, steadiness, elevation, depression, and steerage of vessels for aërial navigation depend have been abundantly verified by experiments both upon a large and small scale. I made a machine having a surface of 300 square feet, which was accidently broken before there was an opportunity of trying the effect of the propelling apparatus, but its steerage and steadiness were perfectly proved, and it would sail obliquely downwards in any direction according to the set of the rudder. Its weight was 56lbs., and it was loaded with 84lbs., thus making a total of 140lbs., about 2 square feet to 1lb. Even in this state, when any person ran forward in it with his full speed, taking advantage of a gentle breeze in front, it would bear upward so strongly as scarcely to allow him to touch the ground, and would frequently lift him up and convey him several yards together.

The best mode of producing the propelling power is the only thing that remains yet untried towards the completion of the invention. I am preparing to resume my experiments upon this subject, and state the following observations in the hope that others may be induced to give their attention towards expediting the attainment of this art.

The act of flying is continually exhibited to our view, and the principles upon which it is effected are the same as those

before stated. If an attentive observer examines the waft of a wing he will perceive that about one-third part towards the extreme point is turned obliquely backward, this being the only portion that has velocity enough to overtake the current passing so rapidly beneath it when in this unfavourable position. Hence this is the only portion that gives any propelling force.

Fig. 8.



· To make this more intelligible let *ab*, Fig. 8, be a section of this part of the wing. Let *cd* represent the velocity of the bird's path or the current, and *ed* that of the wing in its waft; then *ce* will represent the magnitude and direction of the compound or actual current striking the under surface of the wing. Suppose *ef*, perpendicular to *ab*, to represent the whole pressure; *eg*, being parallel to the horizon, will represent the propelling force, and *gf*, perpendicular to it, the supporting power. A bird is supported as effectually during the return as during the beat of its wing. This is chiefly effected by receiving the

resistance of the current under that portion of the wing next the body, where its receding motion is so slow as to be of scarcely any effect. The extreme portion of the wing, owing to its velocity, receives a pressure downward and obliquely forward, which forms part of the propelling force, and at the same time by forcing the hinder part of the middle portion of the wing downward, so increases its angle with the current as to enable it still to receive nearly its usual pressure from beneath.

As the common rook has its surface and weight in the ratio of a square foot to the lb., it may be considered as a standard for calculation of this sort; and I shall therefore state, from the average of many careful observations, the movements of that bird. Its velocity, represented by *cd*, Fig. 8, is 34·5ft. per second. It moves its wing up and down once in flying over a space of 12.9ft. Hence, as the centre of resistance of the extreme portion of the wing moves over a space of 0·75 of a foot each beat or return, its velocity is about 4ft. per second, represented by the line *ed*. As the wing certainly overtakes the current it must be inclined from it at an angle something less than 7°, for at this angle it would scarcely be able to keep parallel with it unless the waft downward was performed with more velocity than the return, which may be, and probably is, the case, though these movements appear of equal duration.

The propelling power represented by *eg* under these circumstances cannot be equal to $\frac{1}{8}$ part of the supporting power *gf* exerted upon this portion of the wing, yet this, together with the aid from the return stroke, has to overcome all the retarding power of the surface and the direct resistance occasioned by the bulk of the bird.

It has been before suggested, and I believe upon good grounds, that very acute angles vary little in the degree of

resistance they make under a similar velocity of current. Hence it is probable that this propelling part of the wing receives little more than its common proportion of resistance during the waft downward. If it be taken at one-third of the whole surface, and one-eighth of this be allowed as the propelling power, it will only amount to $\frac{1}{24}$ of the weight of the bird, and even this is exerted only half the duration of the flight. The power gained in the return of the wing must be added to render this statement correct, and it is difficult to estimate this; yet the following statement proves that a greater degree of propelling force is obtained upon the whole than the foregoing observations will justify.

Suppose the largest circle that can be described in the breast of a crow to be 12in. in area: such a surface moving at a velocity of 34·5ft. per second would meet a resistance of 0·216 of a lb., which, reduced by the proportion of the resistance of a sphere to its great circle (given by Mr. Robins as 1 to 2·27), leaves a resistance of 0·095 of a lb. had the breast been hemispherical. It is probable, however, that the curve made use of by nature to avoid resistance being so exquisitely adapted to its purpose will reduce this quantity to one half less than the resistance of the sphere, which would ultimately leave 0·0475 of a lb. as somewhat approaching the true resistance. Unless, therefore, the return of the wing gives a greater degree of propelling force than the beat, which is improbable, no such resistance of the body could be sustained. Hence, though the eye cannot perceive any distinction between the velocities of the beat and return of the wing, it probably exists, and experiment alone can determine the proper ratio between them.

From these observations we may, however, be justified in the remark that the act of flying requires less exertion than from the appearance is supposed.

Not having sufficient data to ascertain the exact degree of propelling power exerted by birds in the act of flying, it is uncertain what degree of energy may be required in this respect for vessels for aërial navigation; yet when we consider the many hundred miles of continued flight exerted by birds of passage, the idea of its being only a small effort is greatly corroborated. To apply the power of the first mover to the greatest advantage in producing this effect is a very material point. The mode universally adopted by nature is the oblique waft of the wing. We have only to choose between the direct beat overtaking the velocity of the current, like the oar of a boat, or one applied like the wing, in some assigned degree of obliquity to it. Suppose 35ft. per second to be the velocity of an aërial vehicle, the oar must be moved with this speed previous to its being able to receive any resistance; then if it be only required to obtain a pressure of $\frac{1}{10}$ of a lb. upon each square foot it must exceed the velocity of the current 7·5ft. per second. Hence its whole velocity must be 42·5ft. per second. Should the same surface be wafted downward like a wing, with the hinder edge inclined upward in an angle of about 50·40° to the current, it will overtake it at a velocity of 3·5ft. per second; and as a slight unknown angle of resistance generates a lb. pressure per square foot at the velocity, probably a waft of little more than 4ft. per second would produce this effect, one-tenth part of which would be the propelling power. The advantage in favour of this mode of application, compared with the former, is rather more than ten to one.

In continuing the general principles of aërial navigation, for the practice of the art, many mechanical difficulties present themselves which require a considerable course of skilfully-applied experiments before they can be overcome; but, to a certain extent, the air has already been made navigable, and no one who has seen the steadiness with which weights, to the

amount of ten stone (including four stone, the weight of the machine), hover in the air, can doubt of the ultimate accomplishment of this object.

The first impediment I shall take notice of is the great power that must be exerted previous to the machine's acquiring that velocity which gives support upon the principle of the inclined plane, together with the total want of all support during the return of any surface used like a wing. Many birds, and particularly water fowl, run and flap their wings for several yards before they gain support from the air. The swift (*hirando apus. Lin.*) is not able to elevate itself from level ground. The inconvenience under consideration arises from very different causes in these two instances. The supportive surface of most swimming birds does not exceed the ratio of four-tenths of a square foot to every lb. of their weight. The swift, though it scarcely weighs an ounce, measures 18in. in extent of wing. The want of surface in the one case and the inconvenient length of wing in the other oblige these birds to aid the commencement of their flight by other expedients, yet they can both fly with great power when they have acquired this full velocity.

A second difficulty in aërial navigation arises from the great extent of lever which is constantly operating against the first mover in consequence of the distance of the centre of support in large surfaces, if applied in the manner of wings.

A third and general obstacle is the mechanical skill required to unite great extension of surface with strength and lightness of structure, at the same time having a firm and steady movement in its working parts, without exposing unnecessary obstacles to the resistance of the air. The first of these obstacles that have been enumerated operates much more powerfully against aërial navigation upon a large scale than against birds, because the small extent of their wings

obliges them to employ a very rapid succession of strokes in order to acquire that velocity which will give support, and during the small interval of the return of the wing this weight is still rising, as in a leap, by the impulse of one stroke till it is again aided by another. The large surfaces that aërial navigation will probably require, though necessarily moved with the same velocity, will have a proportionately longer duration both of the beat and return of the wing, and hence a greater descent will take place during the latter action than can be overcome by the former.

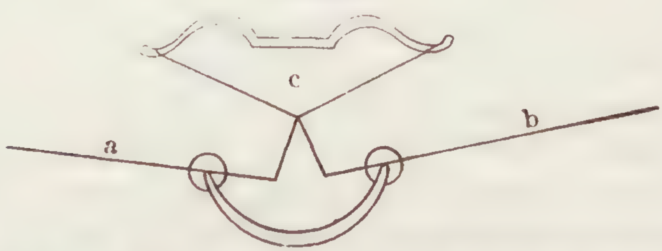
There appears to be several ways of obviating this difficulty. There may be two surfaces, each capable of sustaining weight, and placed one above the other, having such a construction as to work up and down in opposition when they are moved, so that one is always ready to descend the moment the other ceases. These surfaces may be so made, by a valve-like structure, as to give no opposition in rising up, and only to resist in descent. The action may be considered either oblique. as in rotative flyers, alternately so, without any up-and-down waft as in the engine I have described at Fig. 12, a number of small wings in lieu of large ones, upon the principle of the flight of birds, with small intervals of time between each waft, and, lastly, by making use of light wheels to preserve the propelling power, both of the beat and the return of the wings, till it accumulates sufficiently to elevate the machine upon the principle of those birds which run themselves up. This action might be aided by making choice of a descending ground like the swift.

With regard to another part of the first obstacle I have mentioned, viz.,—the absolute quantity of power demanded being so much greater at first than when the full velocity has been acquired,—it may be observed that, in the case of human muscular strength being made use of, a man can exert, for a

few seconds. a surprising degree of force. He can run upstairs for instance with a velocity of from 6 to 8ft., perpendicular height. per second. without any dangerous effort. Here the muscles of his legs only are in action. but. for the sake of making a moderate statement. suppose that with the activity of his arms and body, in addition to that of his legs, he is equal to rising his weight 8ft. per second; if in this case he weighs 11 stone. or 154lbs., he will be exerting for the time, and energy equal to. more than the ordinary force of two of Messrs. Boulton & Watt's steam horses. and certainly more than twelve men can bestow upon their constant labour. If expansive first movers be made use of they may be so constructed as to be capable of doing more than their constant work. or their power may be made to accumulate for a few moments by the formation of a vacuum or the condensation of air, so that these expedients may restore at one time, in addition to the working of the engine. that which they had previously absorbed from it.

With regard to the second obstacle in the way of aërial navigation, viz.,—the length of leverage to which large wing-like surfaces are exposed,—it may be observed that being a constant and invariable quality, arising from the degree of support such surfaces give, estimated at their centres of resistance, it may be balanced by an elastic agent that is so placed as to oppose it.

Fig. 9.



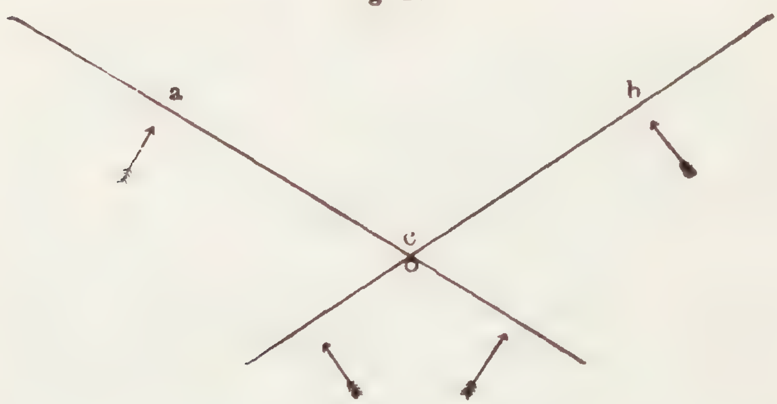
Let *a* and *b*, Fig. 9. be two wings of an aërial vehicle in

the act of skimming, then half the weight of the vessel is supported from the centre of resistance of each wing, as represented by the arrows under them. If the shorter ends of these levers be connected by cords to the string of a bow *c*, of sufficient power to balance the weight of the machine at the points *a* and *b*, then the moving power will be left at full liberty to produce the waft necessary to bend up the hinder edge of the wing and gain the propelling power. A bow is not in fact an equable spring, but may be made so by using a spiral fusee. I have made use of it in this place merely as the most simple mode of stating the principles I wished to exhibit. Should a counter-balancing spring of this kind be adopted in the practice of aërial navigation, a small well-polished cylinder, furnished with what may be termed a bag-piston (upon the principle made use of by nature in preventing the return of blood to the breast, when it has been driven into the aorta by the intervention of the semilunar valves), would, by a vacuum being excited each stroke of the wing, produce the desired effect, with scarcely any loss of friction. I have made use of several of these pistons, and have no scruple in asserting that, for all blowing engines, even friction is an evil, and being very nearly air-tight is sufficient. There is no piston at all comparable with them. The most irregular cylinder with a piston of this kind will act with surprising effect. To give an instance: a cylinder of sheet-tin, 8in. long and 3½in. in diameter, required 4lbs. to force the piston down in 15 minutes, and in other trials became perfectly tight in some positions, and would proceed no farther. The friction, when the cylinder was open at both ends, did not exceed half-an-ounce. These elastic agents may likewise be useful in gradually stopping the momentum of large surfaces when used in any alternate motion, and in thus restoring it during their return.

Another principle that may be applied to obviate this

leverage of a wing is that of using such a construction as will make the supporting power of the air counter-balance itself. It has been before observed that only about one-third of the wing in birds is applied in producing the propelling power, the remainder, not having velocity sufficient for this purpose, is employed in giving support both in the beat and return of the wing.

Fig. 10.



Let *a* and *b*, Fig. 10, be two wings continued beyond the pole or hinge upon which they turn at *c*. If the extreme parts at *a* and *b* be long and narrow they may be balanced, when in the act of skimming, by a broad extension of less length on their opposite side, this broad extension, like the lower part of the wing, will always give nearly the same support, and the propelling part of the surface will be at liberty to act unincumbered by the leverage of its supporting power. This plan may be modified many different ways, but my intention, as in the former case, is still the principle in its simplest form.

A third principle upon which the leverage of a surface may be prevented is by giving it a motion parallel to itself, either directly up and down or obliquely so. The surface *a* 1.

Fig. 11.

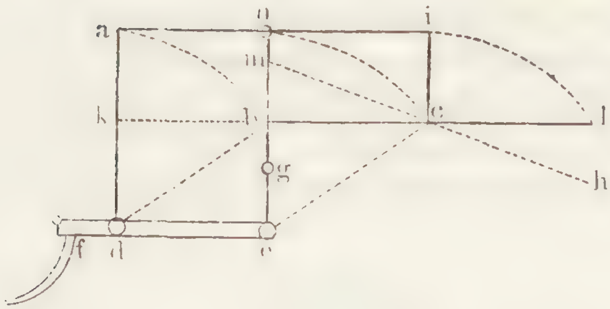


Fig. 11, may be moved perpendicularly by the shaft which supports it down to the position *kc*, or if it be supported upon two shafts, with hinges at *d* and *e*, it may be moved obliquely parallel to itself into the position *bl*.

A fourth principle upon which the leverage may be greatly avoided, when only one hinge is used, is by placing it considerably below the plane of the wing, as at the point *d*, Fig. 11, in respect to the surface *a*. It may be observed in the heron, which is a weak bird with an extended surface, that its wings curve downward considerably from the hinge to the tip; hence the extreme portion which receives the chief part of the stroke is applied obliquely to the current it creates, and thus evades, in a similar degree, the leverage of that portion of the supportive power which is connected with the propelling power. These birds seldom carry their waft much below the level of the hinge of the wing, where this principle, so far as respects the supporting power, would vanish.

By making use of two shafts of unequal length the two last-mentioned principles may be blended to any required extent. Suppose one hinge to be at *f* and the other at *g*, Fig. 11, then the surface, at the extent of its beat, would be in the position of the line *hm*. If the surface *a*l, Fig. 11, be supported only upon one shaft *ne*, be capable of being forced in some degree from its rectangular position in respect to the

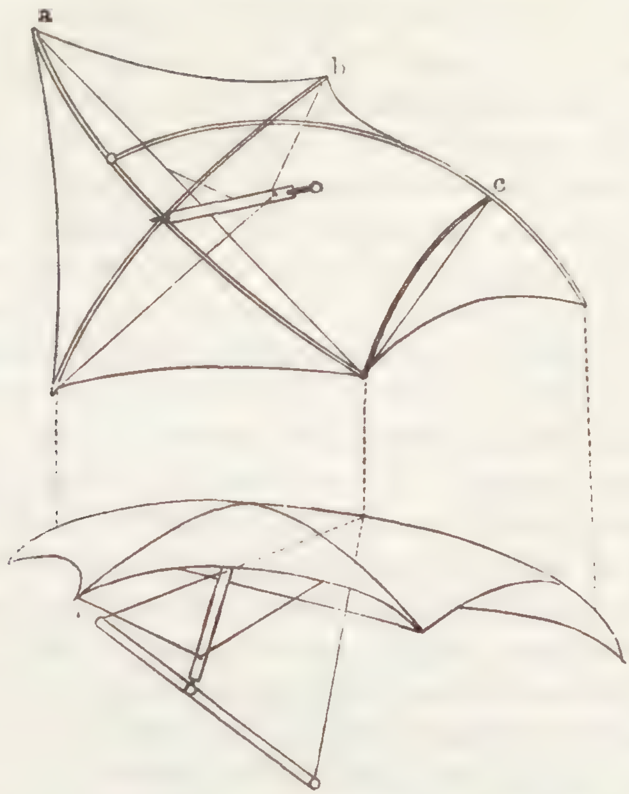
shaft, and be concave instead of flat, as here represented, then the waft may be used alternately backward and forward, according to the principles of the machine I have described at Fig. 12. This construction combines the principles of counterpoising the supporting power of one part of the surface by that of an opposite part when the machine is in the act of skimming, and likewise the advantages of the low hinge, with the principle of leaving little or no interval without support.

A fifth mode of avoiding leverage is by using the continued action of oblique horizontal flyers, or an alternate action of the same kind, with surfaces so constructed as to accommodate their position to such alternate motion, the hinge or joint being in these cases vertical. In the construction of large vessels for aërial navigation a considerable portion of fixed sail will probably be used, and no more surface will be allotted towards gaining the propelling power than what is barely necessary, with the extreme temporary exertion of the first mover, to elevate the machine and commence the flight. In this case the leverage of the fixed surface is done away.

The general difficulties of structure in aërial vehicles (arising from the extension, lightness, and strength required in them, together with great firmness in the working parts, and at the same time such an arrangement as exposes no unnecessary obstacles to the current) I cannot better explain than by describing a wing which has been constructed with a view to overcome them.

Fig. 12 represents the shape of the cloth, with a perspective view of the poles upon which it is stretched with perfect tightness. Upon the point where the rods *a* and *b* intersect is erected an oval shaft, embracing the two cross poles by a slender iron fork, for the purpose of preserving their strength uninjured by boring. To this shaft are braced the ends of the pole *b*, so as to give this pole any required degree

Figs. 12 and 13.



of curvature. The pole *a* is strung like a common bow to the same curve as the pole *b*. and is only connected with the upright shaft by what may be called a check brace. which will allow the hinder end of this pole to heel back to a certain extent. but not the fore end. The short brace producing this effect is shown in Fig. 12. Fig. 13 exhibits the fellow wing to that represented in Fig. 12 erected upon a beam. to which it is braced so as to convert the whole length of it into a hinge. The four braces coming from the ends of this beam are shown : two of them terminate near the top of the centre of the other shaft, the others are inserted into the point *c*, Fig. 12, of the tending rod. A slight bow. not more than three-eighths of an

inch thick, properly curved by its string and inserted between the hinder end of the pole *a* and the curved pole *c*, completes the wing.

This fabric contained 54 square feet and weighed only 11lbs. Although both these wings together did not compose more than half the surface necessary for the support of a man in the air, yet during their waft they lifted the weight of 9 stone. The hinder edge, as is evident from the construction, being capable of giving way to the resistance of the air, any degree of obliquity, for the purpose of a propelling power, may be used.

I am more particular in describing this wing because it exemplifies almost all the principles that can be resorted to in the construction of surfaces for aërial navigation. Diagonal bracing is the great principle for producing strength without accumulating weight, and if performed by thin wires looped at their ends, so as to receive several laps of cordage, produces but a trifling resistance in the air and keeps tight in all weather. When bracings are well applied they make the poles to which they are attached bear endwise. The hollow form of the quill in birds is a very admirable structure for lightness combined with strength where external bracings cannot be had, a tube being the best application of matter to resist as a lever; but the principle of bracing is so effectual that if properly applied it will abundantly make up for the clumsiness of human invention in other respects; and should we combine both these principles, and give diagonal bracing to the tubular bamboo cane, surfaces might be constructed with a greater degree of strength and lightness than any made use of in the wings of birds.

The surface of a heron's wing is in the ratio of 7 square feet to a lb. Hence, according to this proportion of wing of 54 square feet, it would weigh about $7\frac{3}{4}$lbs. On the contrary, the wings of water fowl are so much heavier that a surface of 54 square feet, according to their structure, will weigh $18\frac{1}{2}$lbs.

I have, in these instances, quoted nearly the extreme cases amongst British birds; the wing I have described may therefore be considered as nearly of the same weight in proportion to its bulk as that of most birds.

Another principle exhibited in this wing is that of the poles being couched within the cloth so as to avoid resistance. This is accomplished by the convexity of the frame and the excessive lightness of the cloth. The poles are not allowed to form the edge of the wing, excepting at the extreme point of the bow, where it is very thin, and also oblique to the current. The thick part of this pole is purposely conveyed considerably within the edge. In birds a membrane covered with feathers is stretched before the thick part of the bone of the wing, in a similar manner and for the same purpose. The edge of the surface is thus reduced to a thickness of a small cord that is sewn to the cloth, and gives out loops whenever any fastening is required. The upright shaft is the only part that opposes much direct resistance to the current, and this is obviated in a great degree by a flat oval shape having its longest axis parallel to the current.

The joint or hinge of this wing acts with great firmness in consequence of its being supported by bracings to the line of its axis, and at a considerable distance from each other; in fact the bracings form the hinge.

The means of communicating motion to any surfaces must vary so much, according to the general structure of the whole machine, that I shall only observe at present that where human muscular action is employed the movement should be similar to the mode of pulling oars, from which any other required motion may be derived. The foot-board in front enables a man to exert his full force in this position. The wings I have described were wafted in this manner, and when they lifted, with a power of 9 stone, not half of the blow

which a man's strength could have given was exerted, in consequence of the velocity required being greater than convenient under the circumstances. Had these wings been intended for elevating the person who worked them, they should have contained from 100 to 150 square feet each, but they were constructed for the purpose of an experiment relative to the propelling power only.

Avoiding direct resistance is the next general principle that is necessary to discuss. Let it be remembered, as a maxim in the art of aërial navigation, that every lb. of direct resistance that is done away will support 30lbs. of additional weight without any additional power. The figure of a man seems but ill calculated to pass with ease through the air, yet I hope to prove him to the full as well-made, in this respect, as the crow, which has hitherto been one standard of comparison, paradoxical as it may appear.

The principle that surfaces of similar bodies increase only as the squares of their homologous lines, while their weights, or rather solid contents, increase as the cubes of those lines, furnishes the solution. This principle is unanimously in favour of large bodies. The largest circle that can be described in a crow's breast is about 12 square inches in area. If a man exposes a direct bulk of 6 square feet the ratio of their surfaces will be as 1 to 72, but the ratio of their weight is as 1 to 110, which is $1\frac{1}{2}$ to 1in. in favour of the man, provided he were within a case as well-constructed for evading resistance as the body of the crow; but even supposing him to be exposed in his natural cylindric shape, in the foreshortened posture of sitting to work his oars, he will probably receive less resistance than the crow.

It is of great importance to this art to ascertain the real solid of least resistance when the length or breadth is limited. Sir Isaac Newton's beautiful theorem upon this subject is of

no practical use, as it supposes each particle of the fluid, after having struck the solid, to have free egress; making the angles of incidence and reflection equal. Particles of light seem to possess this power, and the theory will be true in that case; but in the air the action is more like an accumulation of particles, rushing up against each other in consequence of those in contact with the body being retarded.

The importance of this subject is not less than the difficulties it presents. It affects the present interests of society in its relation to the time occupied in the voyages of ships. It will still have more effect when aërial navigation, now in its cradle, is brought home to the uses of man. I shall state a few crude hints upon this point, to which my subject has so unavoidably led, and on which I am so much interested, and shall be glad if in so doing I may excite the attention of those who are competent to an undertaking greatly beyond my grasp.

Perhaps some approach toward ascertaining the actual solid of least resistance may be derived from treating the subject in a manner something similar to the following:—Admit that such a solid is already attained, the length and width being necessarily taken at pleasure. Conceive the current intercepted or disturbed by the largest circle that can be drawn within the given spindle, to be divided into concentric tubular laminæ of equal thickness. At whatever distance from this great circle the apex of the spindle commences on all sides of this point the central lamina will be reflected in diverging pencils, or rather an expending ring, making their angles of incidence and reflection equal. After this reflection they rush against the second lamina and displace it. This second lamina contains three times more fluid than the first; consequently each pencil in the first meets three pencils in the second, and their direction after the union will be one-fourth of the angle

with respect to the axis which the first reflection created. In this direction these two laminæ proceed till they are themselves reflected, when they (considered as one lamina of large dimensions) rush against the third and fourth, which together contain three times the fluid in the two former laminæ, and thus reduce the direction of the combined mass to one-fourth of the angle between the axis and the line of the second reflection. This process is constant, whatever be the angles formed between the surface of the actual solid of least resistance at these points of reflection and the directions of the currents thus reflected.

From this mode of reasoning, which must in some degree resemble what takes place, and which I only propose as a resemblance, it appears that the fluid keeps creeping along the curved surface of such a solid, meeting it in very acute angles. Hence, as the experiments of the French Academy show that the difference of resistance between the direct impulse and that in an angle of six degrees, on the same surface, is only in the ratio of 10 to 4, it is probable that in the slight difference of angles that occur in this instance the resistances may be taken as equal upon every part, without any material deviation from truth. If this reasoning be correct it will reduce the question, so far as utility is concerned, within a strictly abstract mathematical enquiry.

It has been found by experiment that the shape of the hinder part of the spindle is of as much importance as that of the front in diminishing resistance. This arises from the partial vacuity created behind the obstructing body. If there be no solid to fill up this space a deficiency of hydrostatic pressure exists within it, and is transferred to the spindle. This is seen distinctly near the rudder of a ship in full sail, where the water is much below the level of the surrounding sea. The cause here being more evident and uniform in its

nature may probably be obviated with better success, inasmuch as this portion of the spindle may not differ essentially from the simple cone. I fear, however, that the whole of this subject is of so dark a nature as to be more usefully investigated by experiment than by reasoning, and in the absence of any conclusive evidence from either, the only way that presents itself is to copy nature; accordingly I shall instance the spindles of the trout and woodcock.

CONCLUDING REMARKS.

We regret to have to record the death of Mr. F. D. Artingstall, of Manchester, a contributor of interesting results in experimental aviation and a valued correspondent ever since the formation of this Society.

It will be in the recollection of some of our earlier Members that Mr. Artingstall was the Author of a Paper read before a General Meeting and published in the First Annual Report, detailing some experiments conducted by himself.

A *résumé* of these trials will be interesting at this time, as the object which he was endeavouring to accomplish by the aid of steam has lately been obtained by the torsion of India-rubber.

This variation in the experiment, admitting of more ready manipulation within the bounds necessary for the accurate adjustment and observation of wing-action as to angle, contour, and material, may yet conduce to the accomplishment by steam of the results which Mr. Artingstall only partially attained.

He says, speaking of locomotive engines,—"Seeing the vast amount of power in those machines, with more zeal than science I thought it would be no very difficult task to make an engine to fly by steam. I soon contrived one, but under the common impression that it *only* required power and lightness to accomplish flying, which is but true to a certain extent. Another popular error I had fixed in my mind, namely, that for the up-stroke the wings must be valvular, to let the air through."

He then made a model engine, described in the First Report.

"When all was ready for a trial I suspended the machine by a cord from the ceiling of a room to about 5ft. from the floor, then got up steam, and allowed it to accumulate so that there would be a good pressure to start with. When the steam was turned on, the wings worked vigorously, but the machine jerked up and down, whirled round, rushed from side to side, and in fact performed all kinds of gymnastic movements within its limits (except flying), to the great amusement of the particular friends invited to witness the experiment."

Eventually the boiler exploded. Being repaired—"I said to myself, if my engine will not absolutely fly, what amount of gravity will be overcome by the action of its wings? To ascertain this I suspended the machine from the end of a long balance or scale-beam, so that I could counterbalance it with weights at the opposite end, but I found on trial that the up-stroke of the wings drove the engine down and the down-stroke up, so that when at work it beat up and down violently. This agitation of the balance prevented me from ascertaining, with any degree of accuracy, even the effect of the down-stroke. I therefore concluded that the engine should work four wings instead of two; they would thus counteract each other and keep up a continuous buoyancy, as one pair of wings would be going up whilst the other would be descending; but this plan I never carried into operation, for a second explosion ruined my engine, but I now know that it would not have succeeded."

He then constructed a set of vanes, fixed at the top of an upright shaft and driven horizontally by high pressure steam, thinking that the whole might ascend, trying various shapes and dimensions of vanes or screws, ultimately urging the engine to great pressure until the joints were beginning to leak and the connecting rods to bend; all to no effect, except to prove that enormous power is not necessary for flight, as the whole did not exceed the weight of a goose, and although there was

exerted the power of a whole flock of geese its buoyancy was below the weight of one. He then details two experiments as follows:—

"The first was this: I made an engine to work by compressed air, but, notwithstanding its great power compared with its weight, no satisfactory results were obtained. I considered that the wings were too short and broad; I therefore made a pair of long narrow ones, which made the thing look somewhat like a swallow on a large scale. When this was set in motion by a very moderate pressure, it felt as if it would launch off from my hand into the air; however it did not do so, but when I let it go off it descended to the floor with a slow motion. something like a sheet of paper or a wounded bird. I thought the reason of its not actually flying was the want of more angular action or greater sweep of wing, and more power, so I altered it accordingly. I then tried it, but to my astonishment found that it would fly little better than a tailor's goose; thus the greater power and sweep had made it far worse. This I accounted for on the principle that, in the first instance, the power and sweep corresponded better to the size and speed of the aërial waves or pulses excited by that peculiar shaped wing. Not being able again to obtain the same results, and finding that compressed air was very irregular in its action and of short duration, to test the principle further I made another engine to be moved by steam. Its construction was as follows:—On the top of a small but strong steam-generator I screwed a steam-tight movable joint; to this joint was secured a long brass pipe about three-eighths in internal diameter, and to the end of this pipe I fixed my engine and wings only (*i.e.*, not the boiler). The brass tube gave no support to the engine, for it was jointed to the top of the steam boiler as before stated, and in some measure represented the string of a kite, only it conveyed steam

to the engine. When all was ready the generator was put on the fire of the smith's forge. The engine and wings, at the end of the long pipe, rested on a post or stump about 2ft. from the ground. I turned the steam on at the generator, when, to my great satisfaction, *the engine instantly flew into the air, and kept itself up to the length of its tether*. I increased the power of the steam until the wings began to emit a drumming sound, when suddenly they both broke off close to the engine, which of course came down like a stone. I now became weary of these experiments, which produced me neither honour nor profit, but quite the reverse." * * * *

Mr. F. W. Brearey states that in the numerous experiments which he has tried with wings for the purpose of illustrating his Lecture he has made an effective model of what had previously proved to be a failure, by simply reducing the arc of vibration of the wing. Mr. Artingstall was very near to a success at the time he abandoned that particular experiment. Our French *confrères* have quoted him upon several occasions, and perhaps these reminiscences will be interesting to them as well as to us.

His last Letter to the Honorary Secretary is dated the 18th February, 1877. The following is an extract :—

"Notwithstanding I consider Aëronautics to be looking well, yet a strange apathy has come over me on the subject. The words of a clever but rather fast young friend of mine come forcibly to my recollection, although he went to his long home more than thirty years ago. One day he had been assisting me at an aëronautical experiment, and seeing him the following day I asked him 'What he thought ought to be the next move?' He replied, 'If I were in your situation the next move would be to stamp my foot upon the model, throw it to the scrap metal, and wash my hands of the whole business for

ever.' 'Oh,' I said, 'just your prompt mode of despatching business.' 'Well,' said he, 'I'm off to my dinner. You come and dine with me and I will give you my reasons for this advice.' So we posted off.

"After dinner he began thus—'Now, Artingstall, I have too much respect for you to allow you to make a fool of yourself if I can prevent it. I acknowledge that you have made a complete convert of me, and I believe that *practical* flight will ultimately be accomplished; and if you could make an aërial machine that could *practically* fly, *that* would be a good spec, and I should say astonish the natives with it as soon as possible; but experience has shown that such inventions as a rule require years, nay ages, to perfect (the steam engine for example), and that the pioneers of them lose their lifetime and money without either honour or profit, but just the reverse. Though they may make discoveries *essential* to subsequent success, yet even these discoveries will not be appreciated until success has crowned the undertaking, and perhaps the discoveries themselves may be put in other language and form so as to be "cabbaged" by some enterprising "scientific" man to glorify himself.' My reply to all this was 'If your theory had always been acted upon we should have remained savages or barbarians.' He acknowledged this, but said 'He would let those who had a fancy for it do as they had a mind, but he was not ambitious that either himself or friends should become pioneers to science without any immediate benefit.' I saw much truth in his remarks, and soon after abandoned aëronautics until the Aëronautical Society was formed.

"Say if I can in any way assist you in your Lectures on Aëronautics.

"I have tried the French aërial toys. The power required is enormous compared with the small weight raised and the short duration of flight. Unless *greatly* improved, to speak of

them as models for practical flight is simply preposterous. Well might M. Pęnaud fear that it will be many years before aërial navigation will be realized. I expect before long to show something better than aëroplanes and screws, or even wings driven by the *irregular power* of rubber."

It is melancholy to have to bear witness to the truth of the foregoing remarks; nevertheless we are certain that there are workers amongst us who will not be deterred by them any more than was Mr. Artingstall.

He further remarks in another place that "there is no cessation of buoyancy during the up-stroke of the wing, but the body of the bird, while the wings are in action, is as perfectly buoyed up as if it were the car of a balloon."

In the models previously referred to, this perfect buoyancy and direct horizontal flight are shown to perfection.

Until M. Penaud devised the simple mechanial means of vibrating the wings we were unable to imitate the flight of the bird. We can now, however, observe at our leisure the action of different shapes and dimensions of wing surface. We can satisfy ourselves that flight can be performed without calling into action any valvular system of feathers. We shall soon ascertain whether any opening out of the feathers in the up-stroke can by any possibility be made effective in imitative flight.

Mr. Artingstall in one of his Papers says that "the wings of all flying animals have a compound vibration, viz.—what is commonly called the up-and-down stroke, and also the vibration of the wing on its front edge, thereby causing the wing to traverse a kind of wave track. This motion produces a powerful pulsation of the air, perhaps like waves of sound, which gives buoyancy to the bird, and is totally independent of waftage or the common resistance of air. All flying animals," he says,

"drive a current of air from the front edge of the wings to the back, as may be proved by presenting the back edge of a bird's wing, when in motion, to the flame of a candle. This current of air has a lateral pulsation which, by the proper use of the wing, is converted into buoyancy."

All this is demonstrable, and we may say that "the way of the eagle in the air" is no longer a mystery.

Considerable activity, in a quiet and unobtrusive fashion, has been evinced among a few of our Members, and although the results may not for some time to come appear before the public, yet they are sure to produce good fruit. The unhappy state of trade has of course had a very bad effect upon experimental research, for none of our working Members are "millionaires." We are glad, however, to be enabled to say that there is not the least sign of "surrender" among these industrious and indefatigable workers with brain and fingers.

In the opinion of some, the Society ought to be a participator in the fund allotted for the endowment of scientific research.

There are those amongst us who are capable of labouring for the advance of science without hope of ultimate pecuniary recompense.

The institution of the Aëronautical Club has been productive of some interesting intercourse amongst some of the practical workers and those of the Members who are inclined to show more than ordinary interest in their labours. The Club is supplementary to the Society, but in its working quite independent. It meets from October to May inclusive, the third Tuesday in each month, and to these Evening Meetings none are admitted except Members of the Aëronautical Society. at an additional Subscription of 10s. 6d.

E F.

Mr. Fred. W. Brearey would feel great satisfaction in handing over to the Widow of the late Mr. Artingstall any Contributions which may be entrusted to him in response to the following communication :—

"CHEETWOOD, MANCHESTER,
"*July* 16, 1877.

"Mr. BREAREY,

"SIR,—I have to announce to you the death of Mr. Frederick Artingstall, of 248, Collyhurst Road, in this City, and to state that, owing to a great extent to his scientific pursuits, he has left his Widow and a helpless Daughter (subject to fits) totally unprovided for.

"If the Noblemen and Gentlemen with whom you are associated in the Aëronautical Society could kindly contribute a small sum to relieve her present and very pressing necessities, you would be conferring a kindly and truly-charitable deed.

"I have been acquainted with the late Mr. Artingstall for the last 35 years, and can bear testimony to his peaceful and conscientious character during the whole of this period.

"I am, Sir,
"Yours very respectfully,
"ELIAS NATHAN.

"FRED. W. BREAREY, Esq.,
"*Blackheath.*

"Mrs. Artingstall's address is 248, Collyhurst Road, Manchester."

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The following SPECIFICATIONS OF PATENTS

ARE PRESENTED TO THE SOCIETY BY THE COMMISSIONERS.

Date. 1876.	*No.*	*Subject.*	*Patentee.*
Jan. 27.	327.	Improvements in transmitting motion on, or in, water, the air, or on land, and in the means or apparatus employed therefore	J. Buchanan.
Feb. 3.	439.	A new Machine to travel along a line attached to a kite, or any high point, and carrying up and dropping any material placed thereon, the machine returning to the hand	J. J. Snow.
June 8.	2393.	A new improved method of directing and controlling Balloons...	M. Runkell.
July 11.	2827.	Improvements in producing motive power, in the application of such improvements to useful purposes, and in the Apparatus necessary for effecting the same	E. H. C. Monkton.
July 28.		A new or improved Flying or Aërial Toy—Communicated by La Société Dandrieux Gravier et Cie.	

BOOKS. PAMPHLETS. &c., RECEIVED.

Les Nouveaux Ballons, par Arsène Olivier—several copies—By the AUTHOR.

Navigation Aërienne, par M. P. Cordenons, Professeur de Matématiques, de Lycée de Rovigo—By the AUTHOR.

Smithsonian Report for 1874—By the BOARD OF REGENTS, WASHINGTON.

The Monthly Numbers of L'Aëronaute—By M. DE VILLENEUVE.

Twelfth Annual Report

OF THE

AËRONAUTICAL SOCIETY

OF

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FOR THE YEAR 1877.

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FOR THE YEAR 1877,

Containing an Account of the Proceedings and a Selection from the Papers and Communications received by the Society during the year, with Concluding Remarks upon the present state of the Science.

THE ANNUAL MEETING OF MEMBERS of this Society was held in the Room of the Society of Arts, Adelphi, by the usual kind permission of the Council, on the Evening of the 13th of June; Mr. JAMES GLAISHER, F.R.S., in the Chair.

The Minutes of the preceding Meeting were taken as read.

The CHAIRMAN: Ladies and Gentlemen,—I am sorry to say that the Duke of Argyll has informed our Secretary that he has been engaged for this night for two months, or he would have been here. I wish he had been present, for it is now some time since he occupied this Chair. It is two years since I occupied it. At that time I spoke enthusiastically of Mr. Moy's Aërial Machine. I was in hopes that as on one occasion it raised 120lbs., we might by this time have advanced to a multiple of that weight. I met Mr. Moy some time ago, and he said he had been so much engaged that he had not had time to proceed with his invention, but he had not given up the idea. He said that the only thing that had

prevented him carrying on an investigation of so much interest were matters that absolutely required his attention. His feeling and his heart, he said, were in the cause. It was only this morning that Mr. Brearey saw me and told me that he would probably require me to take the Chair. I have now to call upon Mr. Jay to exhibit a model of the figure 8 movement as a propeller for aërial use.

Mr. JAY exhibited a Model of his invention, and read the following Paper on

THE FIGURE 8 MODEL AS AVAILABLE FOR AËRIAL USE.

In placing this Model before you I venture to suggest that the attention of the Members of this Society should especially be directed to finding or inventing a propeller which will enable us to grasp the air in such a manner as to utilise the power we possess in the steam-engine. We are at present unacquainted with any aërial propeller which does not require an enormous expenditure of power; and when I regard the ease with which many birds raise themselves from the ground, I cannot but think there is much room and opportunity for improvement in our mechanical appliances, and that the difficulty may be overcome.

The model I produce illustrates what I believe to be the best (that is to say, the figure of 8 or sculling) action, but the same movement may no doubt be obtained by a more simple mechanical arrangement. Either a direct lifting, or a lifting and propelling action, may be produced by this arrangement. The motion is obtained by means of two cranked axles revolving in opposite directions, and connected together by rods on which a slide works. The roots of the wings are connected to the

slide by means of universal joints, and the fulcra are also supported on similar joints to ensure freedom of action. The slide is attached by a rod to a crank having a greater throw than the cranks previously referred to, so that the wings may traverse a greater horizontal than vertical space. By this action the fore-edge of the wing is depressed at the commencement of the backward stroke and elevated at the commencement of the forward stroke, thereby avoiding the back pressure which would occur in any other reciprocating motion.

The Chairman (referring to the model which had only one pair of wings): There would be more wings than one?

Mr. Jay: There might be wings beyond those.

The Chairman: Was not that part of your proposition?

Mr. Jay: Oh yes. I have thought for some time past of placing wings behind each other, so that each wing would take a fresh volume of air.

The Chairman: Has not Mr. Moy adopted something of that figure to his machine?

Mr. Moy: No, not exactly that.

At the request of Mr. Brearey the model was passed round and examined by the Meeting.

Mr. Moy (to Mr. Jay): Can you increase the angle?

The Chairman: Do you think the inclination is enough?

Mr. Jay: There might be a little inclination to make the wing fly higher up. The object is to raise the back edge of the wing and enable the back strokes to catch the air and spring it up. I have another arrangement of a similar sort on a smaller scale, that will raise itself with a spring two or three strokes off the ground.

The Chairman: Has any gentleman any remarks to make upon the model? if not I will ask you to return thanks to Mr. Jay. Those in favour will signify the same by holding up their hands.

A vote of thanks was given accordingly.

Mr. F. W. BREAREY, Secretary of the Society, exhibited various models, much to the interest and satisfaction of the audience, and read the following paper on

THE PROBLEM OF FLIGHT.

It has been suggested to me that I should exhibit to the Members of this Society the models with which I illustrated my late Lecture upon the "Problem of Flight," delivered at the London Institution. It will be conceived how the subject was handled, and how it led to the consideration of the possibility of its imitation by man. In other words it was a Lecture upon Aërial Navigation, and I almost think it was the first public Lecture upon such a subject delivered before a London audience.

I had previously announced that I was prepared to deliver a Lecture upon "Aërial Navigation," but it was significantly suggested that the title had better be the "Problem of Flight." So under that title I addressed a crowded audience, whose wrapt attention was somewhat remarkable.

The profound ignorance which has prevailed, not only amidst the mass, but amongst men of eminence in other scientific studies, as to the principles upon which the students of our special branch of science depend for the ultimate accomplishment of Aërial Navigation, induced me to turn Lecturer.

The literature upon this subject has been greatly multiplied since the formation of this Society in 1865. The first Report was issued in 1867. In 1868 the French Aëronautical Society published their first Report, which took the form of a monthly bulletin. Our Aëronautical Exhibition in that year at the Crystal Palace was the occasion which led to its issue. The

first numbers were chiefly devoted to notices connected with that Exhibition to which their Society sent a Commissioner in the person of their talented Hon. Secretary, M. de Villeneuve. The Report has been issued monthly ever since. Their Members seem to take more personal interest in the subjects brought forward than do ours. It may perhaps be only conjecture. Certain it is that their invention seems more stimulated, and has taken the form of some very effective models with which they illustrate flight.

These were all that I required in demonstration of what I had to advance in my Lecture.

I have heard it said by one of the Members of our Society that we shall never learn anything from models. This from a gentleman who is always going to construct a large apparatus, but has not yet commenced it. I, however, emphatically deny the proposition.

Taking these French models as my foundation I have constructed, re-constructed, and improved upon them. My mind has been stored with significant but delicate facts which would altogether have escaped my notice but for the action of the models; and the papers which have been read before the Society in this room have been invested with a new interest.

Gentlemen, by your encouragement I have been enabled to act as Honorary Secretary of this Society, and I am thereby placed in a position to extend that encouragement to correspondents and workers in many parts of the world, and I grudge no time spent in contributing to the elucidation of the mystery which man has made of flight. No other subject, except that of daily bread, engrosses my thoughts, and the remainder of my life (accidents alone excepted) will be as the last 12 years.

It is not my intention to deliver a Lecture upon Aërial Navigation to the Members of this Society.

Much of the information which I can give with great

advantages to others, has been within the reach of the Members in the published Reports, and in other publications of which they are no doubt cognizant.

I will therefore commence to show you how I treat the subject in my Lecture; and first I illustrate *flight by projection* by these familiar paper models.

Models projected by the hand.

I then proceed to *flight by gravity* alone, showing how the bat, hanging by its claws, by simply releasing itself attains its first flight.

Liberation of BATS from the top of the room.

Then I show how the application of force neutralizes the force of gravity. In this model the screw propels a plane surface, which here is represented by wings. It is made after the model of M. Penaud, of the French Society, improved as to the screw by myself.

Flight by force and surface.

It is obvious that different forms of surface may be employed here with instructive results for future work. For instance, I have, in this next model, adapted the albatross form of wing, this model being about half the length, viz., 7ft., but the breadth being only one-fourth or two inches, that of the albatross being about 8in.

Flight of albatross model.

This class of experiment may be greatly varied with a view to ascertain the weight which can be carried under a given surface. I think it will be found that the angle of inclination with which the wing advances will have to be increased with the weight, and also the force in the same relative proportion.

I come now to demonstrate the propelling and supporting surface in one, as in the wings of a bird, but first I show the action of a wing as a propeller.

It is asserted by some naturalists, in explanation of this

effective wing action, that the feathers of a bird's wing are made to underlap each other, so that in the downward stroke the pressure of the air closes them upwards against each other and converts the whole series into one connected membrane, through which there is no escape; whilst in the upward stroke the same pressure has precisely the reverse effect. "It opens the feathers," says the Duke of Argyll, "separates them from each other, and converts each pair of feathers into a self-acting valve through which the air rushes at every point." The Duke, in his "Reign of Law," so thoroughly recognizes, in another place, the immense importance of the concave and convex surfaces in gripping the air in the one case and evading it in the other, that I can scarcely think of him as laying much stress upon the valvular system of feathers. Dr. Pettignew, whose researches give weight to his statement, estimates this difference as two to one.

I may perhaps undervalue this valvular theory, and it is possible that, in the case of some birds which appear to have flat wings, the theory may be in part correct, but it is quite certain that in the wing propeller I shall now show you, the convex and concave arrangement is most effective, leaving nothing to be desired.

Wing Experiment.

[Here the Lecturer stood upon a pivoted stool, and holding the artificial wing perfectly level, waved it up and down, by which action he was revolved.]

I will now proceed to the practical application of the concave-convex theory by exhibiting a model after the construction of M. Penaud, of the French Society. The French, as a flighty nation, are fairly entitled to this invention.

I have been experimenting with various forms of wings, and have been enabled to achieve the leisurely flight of the crow and the swift flight of the swallow.

I hope that, after this, we shall hear less about Archytas and his wooden pigeon.

Flapping birds of various kinds.

My Lecture concludes with observations upon the vertical screw, and here again I resort to M. Penaud's Helicoptére in illustration. Some very pretty toys are sold somewhat similar in principle.

I have now gone over the principal topics contained in the Lecture, but I cannot conclude my Paper without some allusion to a subject which has given opportunity for much private comment and some public correspondence, and about which, when lecturing at various places in the Country, my opinion was solicited—I mean the use of Balloons in Polar Exploration.

I shall guard myself against any extreme opinion now, as I did upon those occasions in which I was appealed to. I enter upon the subject with a view to elicit a discussion, as I think that it is a legitimate one for this Society to entertain.

The Balloon has a sphere of its own quite independent of its shape, unapproachable by any other invention, and the question for discussion is—"*Was the late Polar Expedition such an opportunity as afforded any reasonable chance for the useful employment of the Balloon?*"

The first remark that I feel called upon to make is, that unless a Balloon, with the necessary means for its inflation, form part of the vessel's equipment, the world will never learn practically how far its use may be made subservient to Polar Research.

Do there exist any obstacles to the inflation of a Balloon in the Polar Regions with hydrogen gas? Would the moisture evolved in the manufacture of gas convert the envelope of the Balloon into a mass of ice? which I apprehend would be fatal, if irremediable.

I hope to learn that the gas could be caused to enter into the Balloon in a dry condition.

There is still another difficulty that occurs to me, which is that the sulphuric acid would consist of blocks of ice.

I merely mention these as difficulties to be overcome, not forgetting that coal has been discovered in these regions.

All the materials and apparatus being conveyed to the place of destination, there should be no difficulty in the inflation. Giffard's Balloon, exhibited in 1869 at Cremorne, was inflated with pure hydrogen, and could carry upwards of 16 tons.

The Balloon successfully inflated, then what would be the work expected from it?

When I read the Report of that 70 days' journey, to accomplish I believe about 70 miles, at a fearful cost of life and suffering, consequent upon having to drag over ice hummocks, sledges containing provisions, I exclaimed to my friends "why the whole of the stores could have been conveyed over their heads, and the men holding the ropes of this floating observatory would have been assisted by the upward tendency of the balloon." Would the daily consumption of stores compensate the leakage of gas? Major Beaumont, in his history of the Balloon as employed in the American War, says "that the Balloon when inflated can, unless in very windy weather, be very readily carried. Twenty-five or thirty men lay hold of cords attached to the ring and march along, allowing the machine to rise only sufficiently to clear any obstacle. He had frequently," he says, "seen it carried thus without the least difficulty." He further says "that there was always a small amount of leakage, but, from the superiority of the varnish, at the end of a fortnight, sufficient gas remained in the balloon to enable an ascent to be made without its being replenished." The ascensive power of a Balloon thus conveyed for purposes of war must be available at any moment for the two observers, and the additional weight of the two guy ropes which it also

has to sustain, so that the necessity for the twenty-five or thirty men is explained; but for the purposes of exploration and the carrying of stores a very few pounds of ascensional force need be requisite. These stores, however, upon being removed from the Balloon; or the sledges, which might be partly buoyed by the Balloon, being detached, then, could not the Balloon be utilized to survey the Country from some thousand feet or more by means of a let-out cord?

I hope that I am addressing some Arctic Navigators who have been invited here this evening, and who will be able to tell us if any insuperable difficulties exist to prevent the employment of the Balloon as suggested, and also whether upon the organization of the late expedition the subject was considered, and if so, and abandoned, then upon what grounds.

I can conceive how in the hands of a man great in resources, a Balloon, under favourable conditions, could be made a valuable auxiliary, but I cannot conceive how (because there might be a chance that those conditions may not turn out to be favourable) such an adjunct should altogether be left out of calculation.

I can imagine, for instance, the case of a carpenter called into the next street to effect repairs, taking only such tools as he might guess to be necessary, because an omission could be readily remedied, but I cannot imagine him called fifty miles from home without taking his whole basket-full.

Therefore I repeat my proposition—"*Was the late Polar Expedition such an opportunity as afforded any reasonable chance for the useful employment of the Balloon?*"

The remarks of the author and the exhibition of models were much applauded.

The CHAIRMAN: I am sure we must be all much indebted to Mr. Brearey for the beautiful models he has produced, and

which are worthy of a great deal of attention. Perhaps there are some Arctic voyagers here, or some one in the room who can give an answer to Mr. Brearey's proposition. The question Mr. Brearey puts is, "Was the late Polar Expedition such an occasion as to afford a reasonable opportunity for the employment of Balloons?"

Mr. Reece: A surgeon of Plymouth, of the name of Greenway, published in a paper a statement that several months before the Expedition started he did communicate with Captain Sir George Nares, and used the great name of Mr. Coxwell in reference to a suggestion that the use of the balloon would afford a greater extent of vision over the country to be explored than could be obtained in any other way.

The Chairman: Sir George Nares, at the time the Expedition was planned, was in command of "The Challenger," and could hardly have received these suggestions.

Mr. Reece: The writer says he submitted them to Sir George Nares and to the Admiralty, but the suggestions were declined. With regard to hydrogen gas there would be no fear of its efficacy. After it was generated it would pass through ice, or would be so cold that it would maintain the same temperature throughout the journey. Hydrogen gas would be generated at a heat of 180°. It would then pass through a tube and be chilled, and would remain at a temperature of about 32°, so that there would be no fear of its depositing a mass of snow or ice. That objection therefore need not be entertained. There is a proposal made by Mansfield in his work on ballooning, that the weight of a man might be taken off by ballooning. A balloon of 18ft. diameter would take off the weight of a man; anu in this way a man named Ward was able to leap in the forest, from tree to tree, with a velocity of 15 miles an hour. In that case a man

might guide a sledge of dogs at a great pace, and could convey stores by this means to any point.

Mr. Moy: Perhaps Mr. Reece can tell us whether he has had any practical experience of the nature of hydrogen gas in a severe frost.

Mr. Reece: I have made experiments. I have submitted the gas to intense cold, and it appeared to have no effect upon it. I could hardly have expected that it would have any effect. Faraday exposed it to cold 100° below zero and a pressure of 800 atmospheres, and never found that either had the slightest effect upon it. Neither had the most intense cold or pressure that he could produce at the Royal Institution.

Mr. Simmons (the Aëronaut) read the following notes bearing on the subject of the Paper:—

The hot-air balloon seems to be the best adapted to the especial object—

1stly. Because in the presence of intense cold wind does not exist, wind being the chief drawback to the inflation of hot-air balloons in England.

2ndly. Because the more intense the cold of the air surrounding the balloon, the greater the ascending power.

3rdly. The hot-air balloon during inflation will give off sufficient heat from its surface to keep the men warm whilst they are holding the net, and when the balloon is afloat no inconvenience can be experienced from cold.

4thly. The time required for the inflation of the hot-air balloon is about half-an-hour, and the preparation of the apparatus for the inflation will never be found so troublesome or occupy so much time as that for the hydrogen balloon.

5thly. The danger and annoyances from carrying oil of vitriol will be obviated.

6thly. Hot-air balloons have no preparation spread upon their surfaces that can sustain any injury, decomposition, or

spontaneous combustion from being closely packed for a lengthened period.

The entire weight of the balloon apparatus used at the Royal Arsenal, Woolwich, was 1200lbs., its diameter was 70ft., and the heat when inflated, taken 10ft. above the open neck of the balloon, was 120° Fahrenheit.

The greatest difficulty against the inflation of a balloon with pure hydrogen gas in intensely cold regions would be—the keeping of the water in the retort from freezing whilst charging or after being charged with water, until the vitriol is poured in. The process of making pure hydrogen gas by means of furnaces would necessitate the employment of exceedingly cumbersome apparatus.

I should have been pleased to hear the experience of those who had visited the Arctic Regions as to the probable existence of wind during the times when the explorations would be carried on. When I alluded to the non-existence of wind with intense cold, I confined myself to my own experience in Canada.

Mr. Reece: As that gentleman has alluded to the subject of the formation of hydrogen gas, I may say that no one intended to form hydrogen gas by the use of a furnace or by passing over iron filings. It would be produced by pouring one part of sulphuric acid over four parts of water.

Mr. Moy: That freezes.

Mr. Simmons: No, that does not freeze. I am going to ascend at Hyderabad in India by the use of that process.

Mr. Reece: According to the book published by Sir George Nares, the average temperature during the Expedition in the Arctic Regions was 30° Fahrenheit. That would not have the slightest effect on a composition one part sulphuric acid and four water. When you pour that on zinc the temperature would rise to 180° If any one tries that in a

glass vessel he could not keep his hand on it, so that any fear of not generating the gas must be entirely visionary. We must recollect that air expands only one 480th part.

The Chairman: One 491th by the most recent experiments, but one 500th part is near enough.

Mr. Reece: It expands one 480th part, so that you would require great heat for an air balloon. A fire balloon has enormous power, but nothing like one filled with hydrogen gas.

Mr. Moy: The heat would be about 600°.

Mr. Simmons: The heat generated in a hot-air balloon would be 120°. The weight of a balloon and all its paraphernalia might be 1200lbs., the diameter 70ft., and it would carry me into the air if the average heat were 120°.

Mr. Reece: With hot air there would be a danger of setting the balloon on fire if it were composed of varnished silk.

Mr. Simmons: They never are composed of varnished silk; they are unvarnished.

The Chairman: If no Gentleman has any more remarks to make, before asking you to thank Mr. Brearey I would observe that I am not aware myself that the subject of the use of the balloon in the late Arctic Expedition was brought under the notice of the Admiralty, and I do not know that it was taken into consideration at all. I know they were much pressed for space on board the vessels. Everything was excluded that was possible to be excluded on account of the want of room. No communication was made to me. Previous to the undertaking of the expedition, Mr. Francis Galton had written to me in reference to the use that might be made from the whalers, which often proceed very far North, and I advised the use of hydrogen gas balloons. I did not recommend the use of the fire-balloon from the simple fact of the large size

the balloon would have to be. If a balloon of 70ft. diameter had to be taken out, a very large space would be required. Again, it could only be used in Summer time, when there is wind in the Arctic regions. We know that in Russia and Sweden in Winter time, when the temperature approaches zero, it is nearly always calm. To realize the intensity of the cold one must move the hand against the cold air or run against the air. No person standing in an atmosphere 70° below zero would feel that the cold was so intense. It might be far more painful when the temperature was above zero if the air were in motion; but the Winter is not the time when these experiments would be made: they would take place in the Summer, when the temperature would be 40°, and in the sun very much hotter. I see no reason, however, why the balloon should not be made available in various ways in Arctic Exploration, and I do hope that if there is another expedition the balloon will be tried and the question settled. It would certainly, if used in connection with a sledge, enable the distance that could be traversed in the day to be increased. With regard to the view that can be obtained from the balloon: when I was half-a-mile over London I could see Margate and Brighton and on to the Norfolk coast. This shows you how much may be seen from a comparatively small elevation. From the height of a mile you can see nearly ninety miles, and even when a few hundred feet high one is in a position to see over the country for several miles ahead. In any case I hope that in the next expedition, from whatever country it may proceed, not only one balloon but several balloons may be taken out. I need now only express the pleasure we all feel in seeing these models. Mr. Brearey has been working at them for a long time. The beautiful action—the bird-like action—of these models becomes very interesting when we consider that it is produced by mechanism, and I believe that by following up these experi-

ments, even if the problem of flight be not solved, our knowledge upon many points will nevertheless be greatly increased. With these remarks I will ask you to give the warmest thanks you can to Mr. Brearey, because it is to his energy and zeal, ever since this Society was established, that we owe its existence now. He frequently calls upon me, and is always occupied with the investigation of some original and undecided point in our subject. It is to him that I owe the honour and pleasure of being here this evening. He came to me this morning and would not take "No." It is a great thing when a man will not take "No." I gather from your cheers that I need not put the vote. You have already thanked him by acclamation even better than by vote; and that you give him your warmest thanks is proved by your cheers.

Mr. Brearey: I am very much obliged to you for the kind terms in which you have spoken of me. It is the first time in twelve years that I have received a vote of thanks, and I appreciate it the more.

The Chairman: You see heartfelt quiet thanks are not so much appreciated as noisy cheers. The question Mr. Brearey wishes to have put is—"Was the late Polar Expedition such an occasion as afforded a reasonable chance for the employment of the Balloon?" I do not think Mr. Brearey wishes us to find fault with the equipment of the expedition, so that it is unnecessary to put the question as a motion. I will ask Mr. Moy to read his Paper.

Mr. Moy then read the following Paper on

THE CHOICE OF MEANS FOR EXPERIMENTING IN AËRONAUTICS.

It is sufficiently apparent that very many minds are occasionally *exercised* upon the Problem of Aërial Navigation,

and many persons rush into public notice with most crude notions and the barest smattering of mechanical knowledge, and fancy that this problem, coupled with their superficial knowledge, will carry them on to fame and fortune at one bound. It is the constantly unpleasant duty of our worthy Secretary to answer courteously the very numerous applications and proposals that are made to him. One very noisy individual is now happily silenced—at least we hope so—whose plan was so utterly absurd that it only required to be seen to excite ridicule, being to all intents and purposes a hip-bath and a copying-press, with which he was going to "shake the scientific world to its very foundations."

One of the "modern antiques" lately brought very prominently before the public is over 50 years old, and as it has been frequently urged upon our Society, and I am afraid will continue to be so, I wish to say a few words about it.

About the year 1825 it was proposed to surround a balloon with a horizontal sail, capable of being altered by the aëronauts to any angle they pleased, and it was proposed, by the alternate ascent and descent of the balloon, to compel it to travel in any required direction by the dynamic result of the pressure upon the surface of the sail.

This absurd idea has occurred to so many people, and has been brought forward so often, that it is simply a perfect nuisance to have to refute it so frequently; and if its proposers would only study some elementary work on mechanics and calculations on specific gravity, &c., they would themselves see the absurdity of their propositions.

Then there are a number of re-inventions and bright ideas that strike men of all classes and in all lands, who are continually writing to members of our Society, and especially the "Noble Dukes," announcing that they have "solved the problem," and expecting untold gold to result therefrom; but

when the happy interview takes place it is found that they do not even know the pressure of the air at 10 miles an hour, the weight of a cubic foot of air, of a cubic foot of hydrogen or coal gas, or even a cubic foot of water, and as to the cubical contents of a balloon or the cost of the silk they are equally innocent; and although our Reports have been published for 10 or 11 years, these Gentlemen utterly ignore those Reports, and persistently think that their ideas are new as well as good.

There are only a few modes of procedure to choose from. Balloons being the oldest we will begin with them. I will take three of Mr. Coxwell's balloons as examples.

1.—The *Express,* 48ft. diameter, contains 60,000 cubic feet of gas, and will accommodate 7 persons.

2.—The *Nassau,* 52ft. diameter, contains 80,000 cubic feet of gas, and will accommodate 12 persons.

3.—The *Research,* 60ft. diameter, contains 120,000 cubic feet of gas, and will accommodate 15 persons.

This is with common coal gas at, say, 3s. 6d. per thousand cubic feet.

These three balloons give an idea of size and cubical contents suitable for ordinary purposes.

Supposing, then, that a balloon is selected as the subject of experiment, and that you choose a somewhat smaller size, say 30ft. diameter, a globe as here shown on a scale of half-an-inch to a foot. You have a variety of aëriform fluids from which to choose in order to fill it and make it ascend; but bear in mind that you can never make it anything else than a drifting machine. You may try to make it a model of the planet Saturn; you may put to it any amount of sails or other gimcracks; but it will remain nothing else than a drifting machine, able only to ascend, descend, and drift with the wind.

You may take up with another old idea and give it this form—indicated on the black-board—The globe would take

320 square yards of silk, and would contain 14,000 cubic feet of gas, and would displace half-a-ton of air; but this, of 30ft. diameter and 120ft. long, would contain 77,000 cubic feet of gas, and would require 1,260 square yards of silk, the displacement of air being $2\frac{3}{4}$ tons. Here you have a little scope for a very gentle propulsion in calm air, but it is only a little less a drifting machine than the globe. You might carry up two aëronauts and a screw propeller, and do a little feeble work, but it is useless to expect much from this form. It also introduces a new element of difficulty—it requires *stiffening*. This, of course, adds very much to the loss of buoyancy, as you have of necessity added to the weight of materials; and in order to drive this at only 5 miles an hour in still air you would require an engine of at least 3-horse power.

But in order to reduce the resistance still further you may adopt this form—indicated on the black-board—You gain in speed but lose in buoyancy, because of the framing. This has a similar diameter, 30ft., and displacement $3\frac{3}{4}$ tons, but is 184ft. long. 2-horse power would drive this at 5 miles an hour in still air, but it would require a very careful design to make it succeed, and it might possibly be made for £500. or £600.

Now these three forms are all very useful for drifting or travelling very slowly in a calm, but as we cannot abolish wind the wind must be encountered; and as it so happens that with aëroplanes, unsupported by bulky gas, *high* speed means *economical* travelling, most of our Members have come to the sensible conclusion that aëroplanes, properly balanced and driven by steam or other motive power, afford the best means of working out this interesting problem.

I have heard, very lately, from those who have travelled in the Arctic regions, that there is very little wind there in the summer. If this is so there would be no serious difficulty

in constructing an aërial vessel of the form shown by No. 3, and folding it up lengthwise on the deck of a vessel. When the vessel had got as far North as practicable this vessel could be inflated, the engine and propellers attached, and it could certainly be driven at from 10 to 20 miles an hour, *and come back to the ship* with as much precision as any ordinary steam launch can be managed. I make this exception when I call these drifting machines; and the mere fact of such a vessel going 800 miles in the Arctic regions would not deter one Member of this Society from pursuing his work on aëroplane machines, which will dispense with gas or other fluid altogether.

I could not go into the subject of aëroplanes without repeating what has been already published in our Reports, and that would be a waste of your time.

I have been unable to construct any practical machine since my well-known experiments 2 years ago, and must wait until I have means and opportunity. I have some very carefully matured plans which I hope some day to put into practice; and I should be very happy to contract with our Government to fix the British flag where the North Pole is, *or ought to be*, for a very much less sum than that which was expended on the last Arctic Expedition.

The CHAIRMAN: Has any gentleman any remark to make upon the Paper we have just heard. I think the simplest way of stating the floating power of a balloon is that 1,000ft. of gas will lift about 40lbs., the gas being the average gas we are using—14-candle gas. As to the advantages of a fish-shaped balloon, I do not quite agree with Mr. Moy, though we might have power to move with great facility. The advantage of the globular balloon is that we have to use less material, and at present I am inclined to think that the globular form is the best for most purposes. Mr. Moy says "that with the

fish-shaped balloon we could deviate several degrees to the right or to the left;" but I have never been able to satisfy myself that the fish-shaped balloon would succeed. As regards the balloon I am afraid that it cannot be made to deviate much from the direction in which the wind is moving, and I see no means of controlling or evading that strong and great power, the wind—a power for which most aëronauts who have been under its influence, and have had rough descents, will have a great respect. I think our object should be to make some experiments on aërial planes, and if we do not get the knowledge we seek we shall certainly get something that will repay our trouble. If the experimenter does not succeed in the direction in which he hopes for success, he will probably be recompensed for his time and trouble by the knowledge acquired through the experiments themselves. I have now to ask you to thank Mr. Moy for the Paper he has just read.

Mr. Moy: M. dé Lome took up a few men to work his cranks and screw propeller. My engine weighed 80lbs. and exerted 3-horse power; therefore if M. dé Lome had taken my engines he would have done much better.

The Chairman: He did not know your engines. It is a pity that he did not.

Thanks were accorded to Mr. Moy.

Mr. Moy: I must do the same as Mr. Brearey, and thank you for your thanks.

The Chairman: Ladies and Gentlemen, my agenda paper is exhausted, and happily in good time, for I feel that in meetings like this we all regret the approach of ten o'clock. I have only now to adjourn this Meeting, but I have no doubt that new matter will be collected and further information sent out to you with the Annual Report. I thank you very much for the kind attention you have given to the Papers, and hope our Members will give us the benefit of their further researches,

and trust that many investigations and experiments may be made, and the results of them may be communicated to our Society.

Mr. Lefeuvre: I hope this Meeting will not separate without giving a vote of thanks to the Chairman. I can bear testimony to the great interest he has shown in this Society on all occasions, and I was well pleased to see him in the Chair. Our Secretary is a most untiring Secretary, and I hope, with such a Chairman and such a Secretary, before we meet again we shall be able to put before you most interesting facts.

The motion was carried by acclamation, and the Chairman having acknowledged the compliment the Meeting separated.

In accordance with the expressed intention to reprint any matter of interest which might be otherwise unattainable, so that in process of time everything worth knowing upon the subject of Aëronautics might be included in our Annual Reports, we now present a Pamphlet, published in the year 1810, *by Thos. Walker, of Hull, which, by the kindness of Edward Bannister, Esq., J.P., of Grimsby, was lent to the Secretary.*

The publication of this Pamphlet was a matter of history, but it was not known whether a copy was in existence.

A

TREATISE

UPON THE

ART OF FLYING,

BY MECHANICAL MEANS,

WITH A

FULL EXPLANATION OF THE NATURAL PRINCIPLES BY WHICH BIRDS ARE ENABLED TO FLY;

LIKEWISE

INSTRUCTIONS AND PLANS,

FOR MAKING A FLYING CAR WITH WINGS, IN WHICH A MAN MAY SIT, AND, BY WORKING A SMALL LEVER, CAUSE HIMSELF TO ASCEND AND SOAR THROUGH THE AIR WITH THE FACILITY OF A BIRD.

BY THOMAS WALKER,

PORTRAIT PAINTER, HULL.

HULL:

PRINTED BY JOSEPH SIMMONS, AT THE ROCKINGHAM OFFICE; AND SOLD BY LONGMAN, HURST, REES, & ORME, LONDON; AND BY ALL THE PRINCIPAL BOOKSELLERS IN TOWN AND COUNTRY.

1810.

TO THE

Right Hon. Earl STANHOPE.

My Lord,

As far as an obscure individual like myself can judge of exalted characters, I am induced, in unison with public opinion, to hold a belief that your lordship is possessed, in a very superior degree, both of genius and a knowledge of the sciences, as well as a known predilection for every thing that is calculated to improve and extend the mechanic arts, or to meliorate the condition of mankind.

To acknowledge also that your lordship is equally pre-eminent in the senate is but paying a tribute which is *very justly due* to your patriotism, and the great exertions which you have made in advocating the cause of humanity. Every *friend* to his country must hold in grateful remembrance the energetic and manly opposition which your lordship evinced to prevent the commencement of a war more undefined in its object, more inefficient, and more direful and ruinous in its consequences to our country than any war it was ever madly and unjustly plunged into.

My countrymen have *now* great cause also to remember, with indignation and deep regret, that, in return for your opposition to the origin of those baneful effects, which your lordship clearly foretold, and are now but too severely felt; in return for your wise counsels and patriotic zeal, your lordship met with every coarse insult and contumely which blind folly

and malice could suggest. But your lordship has this inestimable consolation, that your life has been most *honourably* engaged—not with the savage arts of murder; not with the burning of towns and the destruction of their unoffending and defenceless inhabitants; not with the filling of Europe with miserable widows and orphans; not with the ruin of manufactures and commerce, and the violation of the sacred constitutional rights and liberties of your countrymen; not with the low, base, and contemptible arts of any corrupt and venal faction; not with the arts of tyranny and oppression, or force and fraud; not with the machiavelian arts; but with the *noble arts* which are conducive to *peace, civilization*, and the *convenience* and *happiness of mankind.*

Had I invented a diabolical engine that would effectually have swept off from the earth a considerable portion of its *unwary* inhabitants, I should never have thought of addressing your lordship; I must have sought patronage from another quarter; but, considering the subject of this work, I thought no one was more able than your lordship to form a just estimation of its merits. I have, therefore, taken the liberty of dedicating it to you, flattering myself that the theory it contains will be honoured with your lordship's approbation, which will greatly contribute to the pleasuve of,

My Lord,

Your Lordship's humble Servant,

THOMAS WALKER.

HULL, FEB., 1810.

PREFACE.

I AM laying before the public a treatise upon a subject perhaps as extraordinary in its nature as anything that has lately come before them; and after a candid perusal, should it meet with approbation from the friends to arts and sciences, my utmost pride will be gratified. The flight of birds, although so common and familiar to our sight, is certainly as great a phenomenon as any in the creation; and artificial flying, when accomplished, may be considered as one of the greatest wonders of the mechanic arts, which I firmly believe attainable upon the plan I have suggested.

In this little work I have shown that birds' wings do not increase their expansion in exact ratio with the increased specific gravity of their bodies; I have given a demonstration of the cause of the projectile motion of birds, the discovery of a true knowledge of which has bid defiance to philosophers in all ages, which, with other discoveries, I trust will prove that I have given consistency to what henceforth may be denominated the *science* of flying, and which may alone be deemed of considerable importance to science, had nothing more than that been brought forward; but as I have gone much further, and have advanced arguments, and given plans to render the *art of flying practicable,* the importance of this little treatise becomes obvious, more particularly so if we take into consideration the various purposes to which artificial flying may be applied.

When my work was just ready for the press, I was much surprised at the account a friend gave me of what he had seen that day upon flying, in a monthly journal. I immediately procured a sight of it, and found it to be an ingenious paper written by Sir George Cayley, and I own I was astonished at the perusal. I conceived it to be very extraordinary that two persons, not having the least knowledge of each other, should be publishing their thoughts at the same time upon such a subject; nor was I less surprised to find the subject treated of there in a manner so rational and far superior to anything I had ever seen before. From what Sir George has thought, and the calculations he has made upon the subject, he is so sanguine in his belief that flying will be effected as to say, in one part of his paper, as follows:—"I feel perfectly confident, "however, that this noble art will soon be brought home to "man's general convenience, and that we shall be able to "transport ourselves and families, and their goods and chattels, "more securely by air than by water, and with a velocity of "from 20 to 100 miles per hour."—*Vide* Nicholson's Journal for November, 1809.

For my own part, whatever reason I may have to be sanguine of success, I have made a resolution to suppress in my work every thought that confidence could suggest beyond what I could give demonstration of, along with the clearest directions how to attain the end in view; thereby putting it out of the power of critics to say that the principles of my theory have not a good foundation.

Notwithstanding, from the novelty and singularity of the subject, I do expect to meet with a good deal of raillery and sarcasm. The wits will tell me that I am flighty, and the more serious and heavy part of mankind, who are too ponderous

for such aërial flights, will express a disapprobation of my scheme; but I do not write for such folks, my sole aim is to deliver my thoughts to the public, in hopes that *men* of *genius* and *science* may turn their attention to a subject that may not before now have attracted their notice, that, by their aid and assistance, the art may be brought into practice; and, as this country stands unrivalled in arts, I hope we shall not be long without a Society for the encouragement of the art of flying. Columbus was laughed at when he talked of a continent beyond the Atlantic; but flighty as he might appear he found it, and *wise* men lost it!

A TREATISE, &c.

WE learn, from several authors, that, in different ages of the world, the art of flying has been attempted by various means, all of which have hitherto failed of success. When we take into consideration the different methods which are recorded to have been tried, we cannot be surprised that they have all failed, since, compared with what is contained in the following pages, they will obviously appear to be nothing more than mere whims and contrivances, all utterly destitute of the true nature and science of flying.

I am conscious that many of my readers, who have never been led to notice the remarks that many eminently-learned men have made upon this art, will be tempted at the first sight of my title page to ridicule a treatise upon artificial flying; for there is not a more common saying, when a person has taken some great difficulty in hand, than that such a thing is as impossible to be done *as for one to fly in the air.* I do assure all such that my treatise is not founded upon a whim of the moment, but from mature deliberation on the display of nature. The study of the works of nature has been to me, during the greatest part of my life, a source of amusement and inexpressible delight. The natural history of birds has particularly occupied my attention, and that enviable faculty which they possess of flying, has greatly excited my curiosity, and led

me to that study by which I have obtained *a true knowledge* of the mechanical principles by which they fly, a knowledge which I do not hesitate to declare has hitherto remained undiscovered, although it has been the object of the study and contemplation of many of the most eminent philosophers of past ages.

That great observer of the works of nature, Solomon, did not overlook the subject of flying, but speaks of it in his book of *Proverbs,* xxx, 18, 19—"There be three things which are "too wonderful for me, yea four, which I know not: *the way* "*of an eagle in the air,* the way of a serpent upon a rock," &c. I beg also to remind such of my readers as doubt the possibility of flying that many useful and valuable mechanical inventions, which are now complete and become common, would, a century or two past, have been treated as visionary or impracticable; or had they been accomplished at such periods their effects would have been attributed to witchcraft. I have not the least doubt of being successful in the art of flying, if I had it in my power to give it a fair trial. My invention for attaining the art is founded *entirely upon the principles of nature;* and although these principles are as old as the creation, they have never, until now, been properly attended to. How much are we indebted to the study of nature for discoveries of the greatest importance? and from this delightful study many more are yet to be expected.

The love of pleasure is natural to man, and to gratify this propensity he eagerly attends to every artificial entertainment that is offered to him. He resorts to theatres and operas, to Newmarket, and other haunts of vanity and folly, as if pleasure were nowhere else to be found; at the same time what an inexhaustible fund of entertainment is overlooked by all but a few, although constantly displayed in the wonderful exhibition of the works of nature.

What a pity it is that minds of men are not more generally and forcibly struck with the pure and tranquil delights resulting from the universal study of nature. What riot, confusion, waste of time, loss of money and of health, might be avoided if this pleasing and truly-enlightening study could be made fashionable. What an infinite stock of ideas it would create; how much it would enrich the human mind, and afford matter for social conversation and entertainment far superior to the unimportant subjects which too generally occupy the minds and tongues of men.

I will now present my readers with some account of various schemes which have been tried to accomplish the art of flying, and shall show the cause of their insufficiency. I shall explain the natural mechanical means by which birds are enabled to fly, and my readers will then be able to judge how far my invention for flying corresponds with the natural science, and is thereby calculated to succeed. I shall show likewise the comparative difference between the specific gravity of the humming bird and the condor, also the different expansion of the wings. I shall compare the weight of a man with the weight of the condor, and thereby determine the necessary dimensions of a pair of wings which would enable a man to fly; and, lastly, I will explain an experiment which I have made, in order to demonstrate the principles of artificial flying, and give directions for making a machine wherein a man may sit, and, by working a pair of wings with a lever, be able to ascend into the air, and fly with as much safety and ease as a bird.

During the early part of my life I have dissected a great many birds, and since studied very minutely the mechanism of their wings, tails, and all the parts which they employ in flying.

I have long been accustomed to contemplate a bird as a living machine, formed by the Almighty creator, either to run upon the earth, to dive in the waters, or to ascend into or fly through the air; and when I examine its various parts, and find such an exquisite display of wisdom in each being formed so perfectly to answer the use it is applied to; when I see the effect of the whole, that such a wonderfully-organized animated piece of matter can quit the earth and soar aloft in the air, it appears to me a miracle, and I am struck with admiration.

It is now almost twenty years since I was first led to think, by the study of birds and their means of flying, that if an artificial machine were formed with wings, in exact imitation of the mechanism of one of those beautiful living machines, and applied in the very same way upon the air, there could be no doubt of its being made to fly; for it is an axiom in philosophy that the same cause will ever produce the same effect.

It is easy to demonstrate that a bird is no more able to fly than a man without the mechanical effect of wings;* therefore, when a man is furnished with a pair of wings large enough, and can apply them in the same manner as a bird does, and with sufficient power, there can be no reason to doubt of a man being able to fly as well as a bird. The machine which I have planned is as close a copy of the natural mechanism of a bird

* The ostrich, in the torrid regions of Africa; the emu, in the extensive plains of Paraguay, in South America, which, standing erect, is about seven feet high, it legs are three feet long, its thighs are nearly as thick as the thighs of a man, it runs so swift that the fleetest dogs are foiled by it; the cassowary and the dodo, in the Molucca Islands; and the penguins, in the Straits of Magellan and the South Sea Islands. All these birds are as utterly incapable of flying as a man, none of them being provided with wings for that purpose.

as artificial means will admit of; and when my readers are made thoroughly acquainted with both the natural and artificial means of flying, I flatter myself they will then be willing to acknowledge that my scheme is a very rational one, highly calculated to insure success in the accomplishment of the art of flying, one of the most extraordinary and desirable arts with which we can be acquainted.

Although I have, for many years, been extremely anxious to bring the machine into effect, and am very sanguine in my expectations of success (for I positively assert that flying cannot be accomplished on any other plan than the one I propose), I, unfortunately, have ever found myself unable, from my professional avocations and other circumstances, to put it in practice, or I should long since have made the experiment.

Finding, therefore, that to no purpose I have deferred, for a long time, its execution, which I deeply regret, and the prospect of the future being not more favourable, I am induced to publish my plan, in the hope that the lovers of the arts and sciences, when I have laid before them a scheme so practicable, will readily be induced, for the honour of science and our country, to contribute to the means of bringing it into practice, and demonstrate to their fellow mortals how they may gain a perfect dominion over another element.

In almost every nation where arts and sciences have flourished, persons have manifested a wish to discover the art of flying. In Rome and in Paris particularly different persons, and in ages remote from each other, have tried experiments with wings formed of various materials, which have been fastened to their arms, but none of them succeeded, there not being strength sufficient in a man's arms to enable him to fly

with detached wings fastened to him, leaving the whole weight of his body unsupported.

Friar Bacon, who lived nearly five centuries ago, wrote upon the subject, and he affirms that the art of flying is possible; and many others have been of opinion that, by means of artificial wings affixed to the arms or legs, a man might fly as well as a bird.

The philosophers of the reign of King Charles the Second were much engaged with this art. The famous Bishop Wilkin, who, in 1672, published a treatise upon flying, was so confident of its practicability, that he says he does not question but that in future ages it will become as common to hear a man call for wings when going a journey as it is now to call for his boots and spurs.

In the year 1709, as we gather from a letter published in France in 1784, a Portuguese, Friar de Gusman, applied to the king to encourage him in the invention of a flying machine. The principle upon which it was constructed, if indeed it had any principle, seems to have been that of a paper kite. The machine was in the form of a bird, and contained several tubes through which the wind was to pass in order to fill a certain sail, which was to elevate it; and when the wind was deficient the same was to be effected by means of bellows concealed within the body of the machine. The ascent was also to be promoted by the *electric* attraction of pieces of amber placed in the top, and by two *spheres* inclosing *magnets* in the same situation.

These silly inventions show the very low state of science at that time in Portugal, especially as the king, in order to

encourage him in his further experiments in such an useful invention, granted him the first vacant place in his College of Barcelos or Santerim, with the first professorship in the University of Coimbra, and an annual pension of 600,000 reis during his life. Of this De Gusman it is also related that, in the year 1736, he made a wicker basket of about seven or eight feet diameter, and covered it with paper, which raised itself about 200 feet in the air, and the effect was generally attributed to witchcraft.

Mr. Willoughby, after observing that the pectoral muscles of a man, in proportion to his weight, are many degrees too weak for flying, recommends to him who would attempt the art with the desire of success to contrive and adapt his wings in such a manner that he may work them with his legs and not with his arms, because the muscles of the legs are much stronger.

The celebrated Lord Bacon wrote on the subject of flying, and believed it practicable, but it seems he could no more direct how it was to be done than any other who had written before him on the same subject.

Thus much, for the satisfaction of my readers, I have thought proper to make mention of what has been attempted in the accomplishment of this wonderful art; but were I to adduce all that has been said and done, at different periods of time, I could compile a large volume of that alone, which would answer no other end than that of curiosity, and to show that no one has ever understood the natural means of flying, which is the only knowledge that can guide us to the completion of artificial flying, and which I hope and trust will be clearly demonstrated in this treatise.

As I shall have occasion to refer to various birds, possessing different powers of flight, in illustration of my design, I here introduce the history of the condor, for the information of such of my readers as may not be acquainted with it.

The condor is a native of America, and hitherto naturalists have been divided whether to refer it to the species of the eagle or to that of the vulture. Its great strength and activity seem to give it a claim to rank among the former, whilst the baldness of its head and neck is thought to degrade it to a rank amongst the latter. It is, however, fully sufficient for our plan to describe its manners, form, weight, expansion, and power; we will therefore leave to nomenclators to decide upon its class. If size (for it is by much the largest bird that flies) and strength, combined with rapidity of flight and rapacity, deserve pre-eminence, then no bird can be put in competition with it; for the condor possesses, in a higher degree than the eagle, all the qualities that render it formidable not only to the feathered tribe, but to beasts, and even to man himself.

Acosta, Garcilasso, and Desmarchais assert that it measures eighteen feet across the wings when expanded; its beak is so strong as to pierce the body of a cow; and it is positively asserted that two of them are capable of devouring that animal. They do not even abstain from attacking man himself; but, fortunately, there are but few of the species. The Indians say that they will carry off a deer or a young calf in their talons as an eagle would a hare or rabbit, that their sight is piercing, and their manners terrific. According to modern authors they only come down to the sea coast at certain seasons, particularly when it is supposed their prey fails them upon the land; that they then feed upon dead fish and such other nutritious substances as the sea throws upon the shore.

Condamine says he has frequently seen them in several parts of the mountains of Quito, and has observed them hovering over a flock of sheep; and he thinks they would, at one particular time, have attempted to carry some of them off had they not been scared away by the shepherds. Labat says that this bird has been described to him, by those who have seen it, as having a body as large as a sheep, and that its flesh is as tough and disagreeable as carrion. The Spaniards residing in that country dread its depredations, there having been *many instances of its carrying off children.* Mr. Strong, the master of a ship, relates that, as he was sailing along the coast of Chili, in the thirty-third degree of South latitude, he observed a bird sitting upon a high cliff near the shore, which one of the ship's company shot with a leaden bullet and killed. They were greatly surprised when they beheld its magnitude, for when the wings were extended they measured thirteen feet from one tip to the other; one of the quill feathers was two feet four inches and three-quarters in length, and an inch-and-a-half in circumference.

Mons. Feuilleé, whose description alone is accurate, has given a still more circumstantial account of this amazing bird.

"In a valley of Illo, in Peru," says he, "I discovered a condor perched on a high rock before me. I approached within gun-shot and fired, but as my piece was only charged with swan-shot the lead was not heavy enough to bring the bird down. I perceived, however, by its manner of flying, that it was wounded, and it was with a good deal of difficulty that it flew to another rock about 500 yards distant on the seashore. I therefore charged again with the ball and hit the bird under the throat, which made it mine. I accordingly ran up to seize it; but even in death it was terrible, and defended itself upon

its back with its claws extended against me, so that I scarcely knew how to lay hold of it. Had it not been mortally wounded I should have found it no easy matter to take it, but I at last dragged it down from the rock, and, with the assistance of one of the seamen, I carried it to my tent to make a coloured drawing of it. The wings of this bird, which I measured *very exactly*, were twelve feet three inches (English) from tip to tip. The great feathers, which were of a beautiful shining black, were two feet four inches long. The thickness of the beak was proportionable to the rest of the body, the length about four inches, the point hooked downwards and white at its extremity, and the other part was of a black jet. The thigh bones were ten inches long, the legs five inches, the toes and claws were in proportion, and the legs were covered with black scales. The little nourishment which these birds find on the coast, except when a tempest throws up some great fish, obliges the condor to continue there but a short time. They usually come to the coast at the approach of evening, stay there all night, and fly back in the morning."

I now proceed to describe the construction and application of the wings of a bird. How properly are they formed to fulfil the uses they were made for! The first is to expand, and by that means to give the bird a secure hold upon the air below it, which hold is always in proportion to the dimensions of the wings. The tail produces the same effect. We see that by means of a pair of wings and a tail duly expanded, in a perfectly *passive state* and aloft in the air, without any muscular motion, a bird procures a suspending power, which counteracts the specific gravity of its body, and prevents it being precipitated to the ground; such is the effect of the wings and tail when in a *passive state*.

I will next take some notice of the quill feathers, which are replete with proofs of the wisdom of the Almighty artist who made them. As they were intended to swim within so light and subtle a fluid as the air is, it was necessary that they should be formed of the lightest materials imaginable; and as they were intended to strike upon the air with great power and rapidity, it was requisite that they should possess in the shafts great strength with elasticity; it was expedient too that the quill feathers should separate and open to let the upper air pass through the wings, to facilitate their ascent when they are struck upwards; it was also necessary that they should all shut close together, forming each wing into a complete surface or web, when they are, by the muscular power of the bird, forced down in order to give a more secure hold upon the air below, and by that means keep the bird up.

Now if we do but examine the quill feathers we shall find in the shafts astonishing strength with elasticity, and very little specific gravity indeed. The webs of the quill feathers are broader on one side of the shaft than the other, which causes them to open as the wings move up and to shut as they come down, exactly answering the purposes I have already mentioned; therefore, we see how wonderfully-complete the wings are in all their parts, and how effectually they serve all the uses required.

I will now show the application and effect of the wings and tail in *an active* state. When a bird, by the power of its pectoral and deltoid muscles, puts its wings into action and strikes them downwards in a perfectly vertical direction upon the air below, that air being compressed by the stroke of the wings makes a resistance, by its elastic power, against the under side of the wings, in proportion to the rapidity of the stroke and the dimensions of the wings, and forces the bird upwards;

at the same time, the back edges of the wings beings more weak or elastic than the fore-edges, they give way to the resisting power of the compressed air, which *rushes upwards past the same back edges,* acting against them with its elastic power, and thereby *causes a projectile force,* which impels the bird forwards; thus we see that by one act of the wings the bird produces both *buoyancy* and *progression.* When the tail is forced upwards, and the wings are in action, the bird ascends, and forced downwards it consequently descends; but the *most important use of the tail is to support the posterior weight of the bird,* and to prevent the vacillation of the whole.

Thus having discovered and explained to my readers the natural mechanical means by which birds accomplish flying, they will be able to see that the plan upon which I have formed my scheme for artificial flying is perfectly analagous to the principles of nature, which certainly ought to be clearly understood, and taken as our only guide, before we can ever expect to arrive at success in the art of flying; but with the knowledge of these principles *there cannot remain a doubt of success.*

When we first think of a man attempting to fly by mechanical means, we are induced, considering his specific gravity to pronounce it impossible; and had we never seen or known of any bird larger than a humming bird, whose weight does not exceed one drachm, and whose diminutive wings measure only three inches from tip to tip; and were to be told by some traveller that he had seen a bird with a body as large as a sheep, that had wings of twelve feet expansion, and that it could quit the earth and ascend into the air with its ponderous body, and there fly about with as much ease as the little humming bird, we should think it too marvellous a tale to be credited. But as we are accustomed to see, almost every day,

birds of such various dimensions and specific gravity as are exhibited by nature, from the humming bird to the common wren; from the wren, through a numerous gradation, up to the eagle, we can readily give credit to the history of the wonderful condor in South America, whose existence is so well attested that we can have no reason to doubt of it, more especially as we witness so vast a gradation in the indigenous birds of our own country. I believe that there were two of these prodigious birds in the Leverian Museum.

The following observations upon the wonderful difference in the weight of some birds, with their apparent means of supporting it in their flight, may tend to remove some prejudices against my plan from the minds of some of my readers. The weight of the humming bird is one drachm, that of the condor not less than four stone. Now, if we reduce four stone into drachms, we shall find the condor is 14,336 times as heavy as the humming bird. What an amazing disproportion of weight! Yet, by the same mechanical use of its wings, the condor can overcome the specific gravity of its body with as much ease as the little humming bird. But this is not all. We are informed that this enormous bird possesses a power in its wings, so far exceeding what is necessary for its own conveyance through the air, that it can take up and fly away with a whole sheep in its talons, with as much ease as an eagle would carry off, in the same manner, a hare or a rabbit. This we may readily give credit to, from the known fact of our little kestril and the sparrow hawk frequently flying off with a partridge, which is nearly three times the weight of these rapacious little birds.

Let us attend to this subject a little further. Let us consider these wings of the condor, which, with a *mechanical*

action alone, produces a power that is capable of carrying through the air both the bird and the sheep, weighing together not less than ten stone, which would then be 204,000 *times the weight of the humming bird!* When this is duly considered, with reference to my plan, what encouragement does it not give to prosecute the art of flying? particularly so when we consider that a man of ten stone weight, in a machine weighing two stone, will only exceed the weight of the condor *one-fifth part;* this is a mere trifle compared with the astonishing difference there is between the humming bird and the condor.

The condor carries ten stone, with wings of twelve feet expansion from tip to tip: the humming bird carries one drachm, with three inches expansion; the common wren is three times as heavy as the humming bird, and has but one inch more of wing; a pigeon weighs 16 ounces, which is 256 times as heavy as it is, and has only ten times more expansion of wing; the goatsucker is forty times as heavy, and has seven times the length of wing. I could here carry the same observations upon other birds to a very great extent, but the above are instances sufficient to prove that birds' wings are not multiplied in their length in the same proportion with the increased weight of their bodies: therefore, as a man weighing ten stone and his machine two, as I have already shown, will only exceed in weight *one-fifth part* of the weight of the condor and his prey; and as the wings of the condor are about twelve feet, suppose we make a pair of wings of silk, one-fifth longer than they are, which will be about fourteen feet and a half, I am thoroughly persuaded they will be found amply sufficient, as they will far exceed the progressive increase of birds' wings.

By attending to the progressive increase of the weight of

birds, from the delicate little humming bird up to the huge condor, we clearly discover that the addition of a few ounces, pounds, or stones, is no obstacle to the art of flying; the specific weight of birds *avails nothing,* for by their *possessing wings large enough,* and *sufficient power to work them,* they can accomplish the means of flying equally well upon all the various scales and dimensions which we see in nature.

Such being *a fact,* in the name of reason and philosophy why shall not a man with a pair of artificial wings, *large enough* and with *sufficient power to strike them upon the air,* be able to produce the same effect.

I shall, after a few observations, proceed to describe how a machine may be made with a pair of wings, and a lever to work them with, so that any person will be able to see how far it is calculated to answer the purpose for which it is intended. This machine may be considered as a large artificial bird, and the man placed in the inside as the vital or moving power. All the attempts hitherto made in the art of flying, by different persons, according to historians, have been mere childish whims, not in the least degree calculated to insure success. They each made a pair of detached wings, some of silk, some of leather, and some of sheet iron and various other materials; they fastened them upon their shoulders or arms: thus equipped, they placed themselves upon some eminence, such as a high tower or a church steeple, then took to their wings; but few of them were fortunate enough to escape without some injury.

It is utterly impossible for a man to fly with a pair of wings fixed to his shoulders or arms, with the whole weight of his body hanging down and depending entirely on his pectoral muscles for support. These muscles in a man are

many degrees too weak to keep extended a pair of wings of sufficient expansion to effectually counteract the specific gravity of his body. Let a man suspend the weight of his body, with his arms extended, holding to an horizontal beam by his hands, and he will very soon find the insufficiency of the strength of his arms to support his weight. On the plan which I have conceived for flying the want of strength in the arms is amply provided for. By furnishing a man with a car to sit in, the whole weight of his body is supported by it, and as he sits much in the same manner as if he were rowing a boat, he is enabled to bring into action his *whole bodily strength,* which *far exceeds* the strength of his arms only, and by sitting in such a position his strength can be exerted with a far greater force than in any other attitude whatever; he at the same time gains an *additional advantage,* in this plan of mine, by exerting his strength upon a lever.

The two greatest requisites for accomplishing the art of flying are these—first, *expansion of wings large enough* to resist, in a sufficient degree, the specific gravity of whatever is attached to them; second, *strength enough* to strike the wings with a sufficient force to complete the buoyancy, and give a projectile motion to the machine. With these two requisites combined *flying must be accomplished;* and, upon my plan, there can be no doubt of wings being made as large as ever they may be wanted; neither ought we to doubt of a man's ability, exerting himself in the way I have described, to bring into action as great a degree of strength, in proportion to his weight, as the condor is possessed of. Therefore, if we are secure of these two requisites, and I am very confident we are, we may calculate upon the success of flying with as much certainty as upon our walking.

When I first thought of artificial flying, it occurred to me that it would be of some importance to try what effect a pair of wings would have upon the air, without any mechanical power to work them; I thought that if I were to suspend a weight from beneath them, and they should prevent that weight from falling in a perpendicular line to the ground, they would demonstrate that the ideas I had conceived of the cause of the projectile motion of birds were well founded.

I therefore made the following experiment, to which I call the *particular attention of my readers, as it positively demonstrates the cause of the projectile motion.* I made a pair of small wings, of fine paper, and very small slips of wood to keep them extended, and fixed on a tail of the same materials, imitating, as near as I could, the wings and tail of a bird when expanded in a *passive state.* I then suspended a small weight from under them, with a piece of thread, exactly in the centre of gravity; I held them up as high as I could reach, then took away my hand and left them flat upon the air, without giving any impulse to them whatever; and by the weight pressing downwards the air under the wings became, in some degree, compressed, and by its reaction against the under side and the back edges of the wings, *they were projected with an oblique descent from one end of the room to the other, carrying the weight all that distance,* which, without the wings being of this particular construction, could not have been done.

I had cause sufficient to exult in the success of my experiment, which proved to me, in a very satisfactory manner, that what I had conceived to be the cause of the projectile motion of birds *was really the cause,* and that if I could but give a vertical motion to the wings, so that they might strike upon the air with a sufficient force, they would then increase

the reaction of the air, and instead of being projected in an oblique descent, totally overcome their specific gravity, *and continue flying in an horizontal direction.*

This is an experiment which any of my readers may make trial of for their own satisfaction and amusement.

Another experiment, serving to shew the different effect of buoyancy obtained by a parachute and by my paper wings, may be tried in the following manner:—Take two straight sticks, neatly dressed, about the thickness of a crow-quill, and each about sixteen inches long, lay them across each other in the middle, at right angles, and tie them fast with a piece of thread; then tie a piece of thread from the ends of one stick to the other, so as to secure them at right angles; then take a sheet of gauze paper, and fasten all the four corners of it to the four ends of the sticks; but previous to this, paste upon the four corners of the paper four small slips of thin cloth, in order to give sufficient strength; then suspend any small weight by a thread from the centre; let the whole fall from a height, and you will see the effect of a parachute in miniature: but this effect is very different from that of the paper wings; the parachute *sinks gradually down in a perpendicular line,* whilst the wings *dart forwards* to the distance of several yards.

I have met with persons who have boldly asserted that it is impossible for a man to exert sufficient strength to raise himself up into the air by mechanical means alone; but the rashness and fallacy of such an assertion is completely refuted and exposed by Mr. Degen, in Vienna, who has very lately actually ascended *into the air,* to a considerable height, by sitting in a machine and giving action to two parachutes; and had he properly understood the principles of birds' wings, and

considered the astonishing power in the reaction of the air, which may be *increased in proportion to any force* exerted upon it, *ad infinitum,* and possessed a complete knowledge of the principles upon which it enables birds to fly, he would have chose wings and not parachutes, and might then have accomplished flying in perfection.*

There is no doubt that, by large parachutes, worked by a mechanical power, a man may raise himself from the ground to a considerable height; but that cannot be properly called flying, because as the compressed air rushes from underneath the parachutes, to regain its equilibrium *on all sides alike,* there will be no *projectile motion* effected, without which *there can be no command or steerage,* and in such case the whole apparatus will be driven which ever way the wind impels it; I therefore cannot give credit to that part of the account of M. Degen's performance which asserts that he flew *in various directions,* although I can readily believe in his having raised himself into the air, and think that great praise is due to him. I do not believe it possible, upon his plan, that he could have gone in any other direction than with the wind; but with a pair of wings constructed and worked according to the natural principles of flying, a projectile motion is obtained in as perfect a manner as buoyancy, *both of which* must be accomplished before we can

* M. Degen, a watchmaker of Vienna, has invented a machine by which a person may raise himself into the air. It is formed of two parachutes, of taffeta, which may be folded up or extended at pleasure, and the person who moves them is placed in the centre. M. Degen has made several public experiments, and rose to the height of fifty-four feet, flying, in various directions, with the celerity of a bird. A subscription has been opened at Vienna to enable the inventor to prosecute his discoveries. – *Vide* the Monthly Magazine for September, 1809.

have the benefit and pleasure of flying with *steerage*, and that upon the following plan only, viz. :—

Make a car of as light materials as possible, but with sufficient strength to support a man in it; provide a pair of wings of about eight feet each in length, let them be horizontally expanded, and fastened upon the top edge on each side of the car, with two joints each, so as to admit of a *vertical* motion to the wings, which motion may be effected by a man sitting and working an upright lever in the middle of the car; *a tail* of about seven or eight feet long, and the same breadth at its extremity, must be fixed to the hinder part of the car, and spread out flat to the horizon in the same manner as we see the tails of birds.

The grebes, by their manner of flying, evince that the most important use of a bird's tail is *to support* the *posterior weight* of the body; for the Creator having left the whole of this class of birds, of which we have five different species, indigenous in this country, all totally destitute of any portion of a tail, they are, consequently, always seen when flying to have their bodies hanging down nearly in a perpendicular direction, and appear to fly with great difficulty; but this impediment in flying is of little consequence to them, their organization being perfectly adapted to their mode of living. They find their subsistence in lakes and pools, wherein they are incessantly diving, and, of course, are not obliged to fly until those places are frozen up, when they are compelled to flutter off, as well as they are able, in search of some spring or swamp which is not affected by frost, where they find a temporary subsistence until their favourite lakes are relieved from a surface of ice; they then return to their former haunts, where they again seem quite in their element. Here we find a class

of birds, owing to their want of tails, possessing the power of flight in a very imperfect degree, compared with some birds. It also may be observed that birds having extraordinary large tails, as the magpie for instance, do not fly in the best manner; none of these birds possess what seems to constitute the excellence of flying, viz., soaring and reposing upon the air; this can only be effected when the weight of the body is upon an equipoise in the centre of the wings and tail, each bearing up its due proportion, and the expansion altogether so large, as to bring the whole weight nearly in equilibrium with the atmosphere. This must be properly attended to in the construction of a flying machine.

To give a further security to the power of suspension, a sail of an equilateral triangle may be spread horizontally over the man's head, supported by a small light mast or bowsprit, at the height of three or four feet above the car; the sail must be expanded and fixed to the mast by a very light yard, presenting the base of the sail to the head of the car, with the opposite point towards the tail, and there fastened with a cord to another small bowsprit; this sail will be a protection, if large enough, in case of any accident occurring to the machine; it will then prevent the man from being precipitated to the ground in a manner similar to a parachute. I only have mentioned this sail that it may be resorted to if it be found necessary in a long voyage; the first experiment I would try without it.

A coachmaker is accustomed to make strong work with little weight of materials; he, therefore, would be the most proper person to make a machine of this kind. The man must sit in the middle, between the wings and the tail, so as to be a little behind the centre of gravity, for the purpose of causing a little preponderance of weight to act upon the back edge of

the wings; for if there be not, in some degree, more weight behind than before, when the compressed air is making a resistance against the underside and back edges of the wings, where it rushes upwards again, causing a great reaction, it would, of course elevate the hinder part of the car too much.

The wings and the tail should be made of silk, very compactly woven, and as impervious to the air as possible. The silk which the wings are formed of, should be laid on in separate broad slips,* and should open to admit the air to pass through as the wings move up, and close together again as they come down, in the same manner as I have described the action of the quill feathers in the wings of birds; although, upon the experiment being tried, this method may not be found so absolutely requisite, for we see flying squirrels, bats, butterflies, beetles, flying fish, &c., with wings formed of compact membranes, all flying exceedingly well. The Madagascar bat has a body the size of a rabbit, with wings four feet long, formed of entire membranes, and, although so large, it can fly as well as our little native bats; therefore it is possible that a pair of artificial wings may be formed without any valves, and yet answer equally well; but this can only be determined by actual trial.

It is necessary to observe that the car in which the man is to sit must be entirely covered on the outside with silk or very thin leather, and along each side of the car the silk or leather must be united to the base of the wings, to prevent,

* The tail feathers of turkies laid close and parallel to each other, and fast sewed upon eight pieces of strong riband, so as to form the same number of slips, then extended in the wing and well braced, would perhaps answer the purpose much better.

as much as possible, the air from escaping any where but from the back edges of the wings: should that be neglected, when the air is compressed by the wings being struck downwards, it will rush upwards through the car and thereby fail of giving that resistance against the underside of the wings which is necessary for the purpose of effecting buoyancy and progression.

I think that the shafts of the wings and tail would answer the purpose in the best manner, if they were each of them made of six long slips of thin whalebone, dressed tapering to a point, then wrapped together in a round form with small twine from end to end, and filled with cork along the inside. By making them in this manner they would spring against the air, would be very light, and so strong that it would be impossible to break them with the power or weight of any one person. By forming them as above we shall humbly imitate the shaft of a quill feather, which is composed of a thin horny shell, containing a delicate light pith along the inside.

I here recommend my readers to *particularly observe* that a *main point in this treatise* is that they should not overlook the importance of the knowledge of the reaction of the air against the underside and *back edges of the wings,* for this is what causes the projectile motion, which is indisputably proved by the flying of my paper wings across a room, and which I will further illustrate by the flight of birds, mill sails, &c.

I have frequently conversed with persons about the art of flying by mechanical means, and generally found them disposed to treat the idea with ridicule. I have asked them if they knew how birds were enabled to fly, and they mostly answered me nearly in the following manner: that birds could fly because it was natural to them, that they were covered with

feathers, which were such light materials as to help them to fly, and that their wings are properly adapted for flying. This was as far as they could explain, which proved that *all* they knew on this subject amounted to nothing. They generally seemed to indulge an idea that there was something in the flight of birds either supernatural or incomprehensible; but I hope my readers will be convinced, by this little treatise, that the art of flying is as truly *mechanical* as the art of rowing a boat.

I will here further illustrate how flying is effected. The air, when struck upon by wings, produces an effect by its reaction against the underside and back edges, similar to that which is caused by the wind blowing with sufficient force against a mill-sail, when it *rushes off on one side,* and impels the sail to move, with this difference only, that the sail, being fastened at one end to an axis, is made to revolve, whilst the bird, being at full liberty in the air, is caused, by the expansive power of the air acting with a resisting force *against the back edges* of the wings, to glide forward in a right line.

Most of my readers, I think, will acknowledge the great elastic power of the wind, as it is manifested by the sailing of ships and the revolving of mill-sails; these effects are produced by the wind being compressed against the sails from its own natural motion and force; but the effect the air has against the wings or sails of birds is produced by its being compressed, with them striking vertically upon it; and the larger they are made the greater quantity of air is compressed, by which means is caused a more powerful reaction, and consequently a more effectual buoyancy and progression. From this cause all the birds whose wings are *very large* in proportion to their weight are able to fly with the *least exertion* imaginable, whilst birds with

very small wings are obliged to use very great labour indeed; this being demonstrated by the examination of the dimensions of birds' wings and their specific gravity, and by observing their different methods of flying.

I have often been delighted with the striking conviction that Supreme wisdom alone could have so nicely adjusted all the various internal and external organization of the vast number of different species of birds, to their diversified wants and modes of living; but it is only necessary to observe here that all those which are under the greatest necessity of flying are provided with the *longest* and *best proportion* of *wings* and *tails*, and are consequently able to fly in the best manner, and those which need them less have them more limited, and are therefore less capable of flying, as if the all-wise Creator had set limits to their powers of flight, that they might not go out of their respective elements.

Although I think that a pair of wings seven or eight feet each in length would be sufficient, still, if I could make it convenient to try the experiment of flying, and were not prevented, as I am, by a chain of untoward and uncontrollable circumstances, I would cause the wings to be made of as large dimensions as I could possibly *move with ease.*

I observe amongst the aquatic birds that the auks, guillemots, divers, &c., have such remarkably small narrow wings that they would be utterly incapable of keeping themselves up in the air if it were not for an exertion which they are obliged to make in the extreme. Their wings are moved with such rapidity as to be with difficulty discerned. In this we see the economy of the all-wise Creator, for according to their habits and appetites they have very little occasion to fly at any time,

except during the time of incubation, when they have to ascend the most inaccessible rocks and cliffs they meet with along the sea shore, where they breed and rear their young; all the rest of their time they pass on or in the water, swimming and diving for their food.

All the gallinaceous class of birds have very short concave wings, which they strike with great exertion; they also, in general, have but little occasion to fly; their food, which consists principally of grain and seeds, being spontaneously scattered over the earth, they are almost constantly upon their legs, running about to pick it up, and seldom fly but to avoid danger.

On the other hand, rapacious birds, who appetites induce them to be the greatest part of their time upon the wing, in search of a subsistence which is very precarious (as every inferior bird, &c., to which they direct their sanguinary attacks, from that love of existence which God has so strongly implanted in all His creatures, will use its utmost skill and activity to elude its destroyer), are much better accommodated, having wings of large dimensions they can repose upon the air, and project themselves forward with a gentle wafting. This is the class of birds I would copy from in the construction of a machine for artificial flying. The kite or glead, P, B, Z, (or *milvus* from Lin.,) is the best natural specimen that we can find in the British ornithology; this bird has very large flat wings, with a large forked tail, and flies with the least exertion, I believe, of any bird in the creation.

All the hyrundo class of birds are almost constantly flying; they all have bodies of little weight, have large *flat* wings, and fly with great ease. The goat-sucker, which is a species of nocturnal swallow, is admirably constructed for flying with facility.

As I have mentioned aquatic birds, I will here take the opportunity of execrating, with all the indignation of my soul, that savage and brutal amusement which they bring to my mind, and which so many persons frequently practice and take delight in ; I mean the shooting these harmless and inoffensive birds. Many are the parties who resort to Flamborough-head, for no other purpose than gratifying their vanity by making a display of their dexterity in shooting, and causing all the havock they possibly can amongst the poor inoffensive birds. Barren must be their minds, and callous their feelings, who can take pleasure in destroying these innocent creatures, which are not in the smallest degree offensive to man when they are living, nor of the least service to him when killed. If these *gentlemen* could eat them when they have done shooting, that would be some excuse ; but as their flesh is very rancid these wanton barbarians have no relish for their game. I wish their humanity was as nice as their appetites, they would then not find delight in merely shooting them for sport and cruelty, leaving them, some killed and others wounded, floating on the surface of the sea, whilst their helpless young ones must consequently perish with hunger upon the shelvings of the rocks. Such amusements, surely, are not becoming rational beings, but may give pleasure to semi-rationals.

In the months of May and June these birds, which, during the rest of their time are dispersed over various parts of the ocean, are brought by one of the great impulses of nature to assemble at Flamborough-head in myriads, producing a throng, upon a great extent of cliff, similar to what we see in miniature in the front of a bee-hive, on a fine summer's day, when there is a perpetual egress and ingress of thousands.

A person who has never seen such a sight, and is capable

of deriving pleasure from contemplating the economy and the works of nature, may find an exquisite gratification in paying a visit, at this season of the year, to Flamborough-head, without having recourse to wanton acts of cruelty. Will there ever come upon the earth a generation of men who will despise all pleasures that are either unreasonable or inhuman?

Reason and *humanity* constitute the *only permanent basis* of all human happiness, and the *real* honour and *true* glory of man! without which he is but a compound of folly and madness, and is too often a vile mischievous brute. By a disregard and contempt of these two divine guides families and nations become distracted and are made miserable, as we have too amply witnessed in the deplorable and wretched state in which Europe has been so long afflicted, where the appetite of the cannibal has *only* been wanting to complete the brutality of *civilized* nations. But I am departing too much from my original subject; I will withdraw my pen from this sickening view of poor, frail, erring, human nature!

After having described how to construct a machine to fly in, which, like the swift or great black martin (*apus*, Lin.), cannot fly from the surface of the ground, but must have an elevation to rise from, it becomes necessary that I should give directions how it may be made to ascend. Set two tressels fast upon the ground, one six feet high and the other four-and-a-half, at twelve feet distance from each other; then lay upon them two or three planks, which will form a stage with an oblique plane, upon which the car must be placed, with its head pointing to the higher end of the stage.

A person may then get into the car, and sit a little behind the centre of gravity, which must be adjusted before the car is

placed there; being thus elevated he will have depth enough on each side of the car to admit of his wings striking upon the air. He must then push the lever forward about eighteen inches from its perpendicular line, the tips of the wings will then rise three feet and a half above the level of their joints; he must then, with a brisk exertion, pull the lever backwards eighteen inches past the perpendicular line, and the tips of the wings will be struck downwards, passing through an arch of seven feet and suddenly driving down and compressing the air in that arch, part of which will escape past the back edge of the wings (as I have described before), making at the same time a reaction which will push the wings forward: and as the car and the wings are first placed on an oblique plane, they will be impelled forwards, making an oblique ascent. The projectile impulse will naturally force the machine upwards in any angle in which the plane of the wings is laid, somewhat similar to what may be observed in the raising of a common paper kite, except in a right angle, or perpendicular line; but the nearer the angle of ascent inclines to the line of the horizon, the easier will the machine be found to ascend. I believe pigeons can ascend very near in a perpendicular line, but such an ascent would be too incommodious for artificial flying.

When the car is brought to a sufficient altitude to clear the tops of hills, trees, buildings, &c., the man, by sitting a little forward on his seat, will then bring the wings upon an horizontal plane, and by continuing the action of the wings he will be impelled forwards in that direction. To descend, he must desist from striking the wings, and hold them on a level with their joints; the car will then gradually come down, and when it is within five or six feet of the ground, the man must instantly strike the wings downwards, and *sit as far back* as he

can; he will by this means check the projectile force, and cause the car to alight very gently with a retrograde motion. The car, when up in the air, may be made to turn to the right or the left, merely by the man inclining the weight of his body to one side.

When I have seen a man sitting in a chair upon a tight rope, with a table before him, spread over with decanters, glasses, &c., &c., and, by his *dexterity alone*, be able to keep himself and all his accommodations exactly balanced there, while he sat smoaking his pipe, apparently at perfect ease; I have been induced to consider the art of managing a flying machine, compared with such a surprizing display of human dexterity, to be very simple; and see no reason why men should not become as expert in navigating the air as the sea.

As some of my readers, who may have little regard for any thing but the *utile*, may be induced to ask, what use will flying be of, when it is attained? I beg leave, in the way of reply, to give the following hints:—I hope it will be granted that flying will be of great use, if by such means we can have our letters, newspapers, &c., conveyed to any part of the kingdom at the rate of forty or fifty miles in an hour; or if that numerous class of mercantile agents who are now denomited riders, henceforth be enabled to glide through the air with great expedition, in flying machines; or if a man, by such means, can take a rope to any mariners in distress along the sea coast, and thereby become the happy instrument of saving their lives; and if the circumnavigator be able to quit his ship, fly and explore the interior parts of a new discovered island, free from the annoyance and hostilities of its rude inhabitants—but it would be tedious to enumerate all the uses to which artificial flying may be applied: it is obvious

enough, that when one man is enabled to fly, thousands may do the same, either on business or pleasure. It may tend greatly to reduce the vast number of horses kept in this kingdom, and by that means a *very great quantity* of land, which is taken up at present in growing hay, oats, and beans, for the support of these quadrupeds, might be then cultivated for the increase of our national stock of subsistence for the population; and I think it is evident that we have great occasion to reduce the superfluous number of those animals, and to employ all the land we possibly can to grow corn, &c., for our own subsistence. It is not improbable, that some persons will ask, if flying and all this can be accomplished; to which I answer, that if my scheme for attaining the art be deemed a rational one, as I hope it will, I think we certainly ought to try the experiment.

After the perusal of this work, I hope my readers will be fully convinced, that all attempts which have been hitherto made in the art of flying have failed, not in consequence of the art being impracticable, but from the natural science of flying having never yet been fully understood. All that has ever been written, and all the experiments that have ever been made towards attaining a knowledge of artificial flying by mechanical means, display a chaos of unsettled thoughts very wide and deficient of the principles of nature; but I hope it will be granted that I have clearly discovered and demonstrated the whole of those principles upon which flying depends, particularly the *cause* of the *projectile motion* of birds. This is a discovery of the greatest importance, for as the air is continually acting, in the manner I have described, against the back edges of the wings, and thereby impelling the bird forwards with great force, *it positively has as much tendency to overcome specific gravity as the expansion of the wings has.*

This is a fact demonstrated very clearly by my paper wings, and by the manner of flying peculiar to some birds, particularly the woodpeckers. When one of these extraordinary birds has struck its wings once or twice upon the air, and thereby produced a projectile impulse sufficient to force it forward to a considerable distance, it instantly contracts its wings *as close to its sides* as when perched on a bough, and continues flying several yards with its wings kept *close* in that position, until the impulse is abating; it then throws out its wings again, gives another stroke or two to renew the impulse, *shuts them up*, and is again driven forward; thus continuing to fly by distinct and separate projectile impulses alone. Here then we see the great importance of a true knowledge of the *cause* of the projectile motion of birds, for this surprising bird does not depend upon a continued expansion of wings to keep itself up in the air, but is kept up and carried forward by the projectile force alone!

The green woodpecker is about the size of a pigeon, and as it is very common in every part of England where wood abounds, many of my readers may have an opportunity of observing its curious method of flying; the same may be observed of the beautiful little goldfinch, and of linnets. Here the physico-theologist, who is accustomed to contemplate the wisdom of God in all His works, might be led to infer that He has caused this deviation from the general method of flying, in order to demonstrate to us the *effect* of the *projectile force,* and that it is one of the *greatest essentials* in the art of flying, and perfectly distinct from and independent of the continued expansion of wings.

When we see pigeons flying *upwards* in the angle of *sixty* or *seventy*, as we do every day, from the streets to the tops of

houses, with the plane of their wings parallel to the line of their ascent, I think they prove in a satisfactory manner the great effect of the *projectile force;* for without we admit this to be the cause of their ascending in such angles, how can we possibly account for it in any other way, upon rational principles?

A stone thrown by the hand, and a ball ejected from the mouth of a cannon, are made to overcome specific gravity, and fly to a great distance; we all know that these are not kept up by wings, but entirely by the projectile force. In fact, it is by the air being made continually to push the bird forwards, which constitutes the main cause of flying.

We must attribute to a total ignorance of the fundamental principles, that the art of flying has not been brought hitherto into common practice; for an art, so practicable as it is, must at any period of time have soon succeeded a discovery, such as I have made; and now that the art appears so very attainable, I hope that every friend to arts and sciences will acknowledge that it ought to have a fair trial.

I shall now conclude my treatise on flying with an appeal to the candour and good sense of my readers, whether the arguments I have used, and the principles upon which I have insisted the art of flying may be accomplished, are not such as give it a just claim to their approbation; for I think I may affirm, without being accused of arrogance, that the art of flying has never before been treated of upon such rational and scientific principles.*

* I will here take the liberty of communicating a few hints, which I conceive to be of importance to the aërostatic science. Now that we

Having now submitted to the good sense of my countrymen the whole of what I intended on the subject of flying, I, for the present, most respectfully take my leave of them, indulging a hope that the prediction of Bishop Wilkins, expressed in a former page, will soon be verified, and trusting

know the true cause of the projectile motion of birds, and I having suggested a plan for producing the same effect by artificial means, we may be able to accomplish what Messrs. Roberts, Blanchard, and others attempted to do, but in vain, entirely from their not possessing a knowledge of this mystery of nature. I am alluding to the steerage of balloons, which they endeavoured, with great labour, to attain, by striking a number of oars *horizontally* against the air; and if we do but take into consideration that the balloon was constantly flying from the air against which they were striking, it does not seem probable that they could, by such means, produce the effect they aimed at.

But if we make a car from the plan which I have laid down in this treatise, and upon a scale large enough to admit of one of Messrs. Mead and Co.'s new invented revolving steam engines, to move the lever with, we then can work, in a *vertical direction*, a pair of *very large wings*, which would produce a *projectile force* sufficient to impel the balloon forwards in any point of the compass to which we might incline it; and by having a large tail fixed to the car, in an universal joint, we should be able to give it any inclination whatever; and when we have thus effected a perfect steerage to balloons, we shall be able to convey a number of passengers to any place of destination with accuracy and safety. But for this kind of navigation the balloon must be much smaller than usual, and perfectly spherical, and the gas should be kept in such a degree as not to have too great a tendency to ascend—it should be so regulated as to float in equilibrium with the atmosphere; the aëronauts could then keep the machine at a moderate height—from fifty to a hundred feet would be high enough for ordinary sailing, and if it was found to be inclining too much upwards, it might be counteracted by holding the tail in a descending direction. One of Mr. Mead's patent steam engines can be made with a one horse power, or equal to the strength of eight or ten men, that will not weigh more than eight stone; and will stand in the small space of four feet by two, with the boiler and all the apparatus belonging to it.

that I shall not be disappointed in the opinion I entertain respecting the patronage which they will extend towards the invention now laid before them. Encouraged by the public, I shall not abandon my purpose of making still further exertions to advance and complete an art, the discovery of the *true principles* of which, I trust, I can with verity affirm to be exclusively my own.

FINIS.

CONCLUDING REMARKS.

We have presented our readers in this Report with a reprint of a Pamphlet by Mr. Walker, a Portrait Painter of Hull, upon which some further remarks are here offered.

This essay, considering the time when it was written, is remarkable, inasmuch as the Author ignores the balloon as an available means of flight, or of traversing the air in any direction at will. His ideas of success are entirely based upon mechanical flight obtained upon the same principles and action as that of birds and animals.

Mr. Walker says that the hold upon the air is always in proportion to the dimensions of the wings. Now there is a necessary condition attached to the support of a bird in flight which tends to equalize the great disparity which exists between the weight and wing surface of various birds, viz., the arc of vibration, and the rapidity with which the strokes are delivered. Take for instance two familiar examples—the rook and the pheasant. The weights and measurements of both birds, taken for this purpose, are as follows:—

The rook, $1\frac{1}{4}$lbs., wing surface 152 square inches, a surface which appears to support the bird very often, in a lengthened gliding flight, without any wing motion.

The pheasant, just over 3lbs., wing surface 137 sq. inches. It is therefore impossible for this bird to use its wings as fixed planes and glide like the rook. What it really does is to work its wings with such rapidity that the vibration produces an audible effect, and shows the great muscular force possessed by the bird, due to its much greater weight. It is nearly three

times the weight of the rook, and has 15 square inches less wing surface.

Mr. Walker next alludes to the necessity that the quill feathers should separate and open to let the upper air pass through the wings, but he afterwards qualifies this assertion. and thinks that a pair of artificial wings may be formed without any valves and yet answer the purpose equally well. Certain it is that flight, after the manner of a bird, is now attained by mechanical models without any opening out of the wing feathers in the up stroke, and that an artificial wing, covered with a continuous membrane, is effective in the upward stroke in propulsion.

This may be tested by waving a wing up and down whilst standing or sitting upon a pivoted stool, say a music stool.

As all impulse applied to a body in the air tends to overcome the action of gravity, so in this sense the upward stroke has a supporting effect.

The Author here also anticipates the published results of modern researches into the mechanical condition of flight, for he says that forward speed "positively has as much tendency to overcome specific gravity as the expansion of the wings has."

He also gives us an example of some calculations which he had made respecting the relative wing surface of various birds, which correspond with some of M. de Lucy's recent conclusions, and which evidence his possession of much shrewd observation beyond the sphere of his avocation, that of a Portrait Painter. The result of his argument relative to the condor, its normal weight and extent of its wing, and its capability of carrying off a sheep, is, if the facts be not exaggerated, entitled to consideration.

Although we have reprinted the Pamphlet it is not thought necessary to reproduce the accompanying plates, which seem to show, by a certain clumsiness in the construction, that the

Author was deficient in the mechanical skill requisite to carry his ideas into successful practice.

A man may be perfectly competent to grasp the truth relative to all the actual requirements for obtaining a fulcrum upon the air, and yet grievously fail in adapting such an apparatus as may fulfil these conditions.

We do not say that this remark particularly applies to Mr. Walker; but, as a fact, every inventor will attack the problem in his own way. His apparatus will be evolved from his own perception of the difficulties to be overcome, and will be more or less a failure or more or less a success, according to his mechanical skill. Generally speaking all other inventions than his own are at least—amusing!

When the subject of aërial navigation is only mentioned in the presence of ordinary persons, it is received as though the believers in its possibility are fit subjects for lunatic asylums. That it is simply a mode of transit which can be accomplished by carefully devised means and appliances, and an expenditure of an amount of cash which is altogether insignificant when compared with the importance of the subject, is the established belief of many; and in the midst of the general want of knowledge which exists in connection therewith, it is most refreshing to take up a work like "Aërial Navigation,"* and to find the subject treated not only without prejudice, but in a most comprehensive and philosophical manner. In fact, before going through many pages, the reader finds that he is imbibing the outcome of a mind of gigantic proportions, whose penetrative powers and felicity of resource are almost unbounded: and in the midst of his pursuit of this one grand subject the Author makes sudden darts into other subjects in such a masterly

* "Aërial Navigation," by the late Charles Blachford Mansfield, M.A., edited by his Brother, Robert Blachford Mansfield, B.A.—Macmillan and Co.; 1877.—*Price* 10*s*. 6*d*.

manner as to greatly enhance one's estimate of his capabilities. Cut off by a sad accident in February, 1855, as the Author was, in the height of his bodily and mental vigour, it is clear, from this unfinished work, that, had he lived, he would have been largely instrumental, even if not himself successful, in solving this problem.

The Author goes into the subject in a most exhaustive manner, scarcely leaving a point untouched. The work is resolved into two great divisions, each of which is subdivided into fifteen heads, and an addition of appendices, under six heads, completes the work.

The first division of the work states the "difficulties" which surround the problem, as follows:—

Chap. 1. Introductory.
,, 2. The problem of flying.
,, 3. The impossibility of propelling balloons, and the first difficulty in aërial navigation—the application of force.
,, 4. The second difficulty—the gas-vessel; its stiffness.
,, 5. The third difficulty—the gas-vessel; its firmness.
,, 6. The fourth difficulty—the rising and falling of the air-craft.
,, 7. The gas-vessel—the question of shape.
,, 8. The gas-vessel—the question of material.
,, 9. The gas-vessel—the question of contents.
,, 10. The air-craft—the question of floatage.
,, 11. The fifth difficulty—the air-craft; the question of level.
,, 12. The question of power.
,, 13. The question of waftage.
,, 14. The question of anchorage.
,, 15. Conclusion. Summary of contents.

The second part of the work consists of "hints for the solution of the problem," and goes over each of the above fifteen heads in a highly-instructive and careful manner.

Chapter 1, part the first, is introductory. The Author attacks the opinions expressed in the then later edition of the "Encyclopædia Britannica;" and we may here mention that the late edition of that standard work, written since the death of our Author, omits all the absurd notions which were contained in the former edition, and thus fully confirms the justice of our Author's remarks.

The "problem of flying" is clearly and forcibly stated. In tackling the question whether a man may or may not have sufficient muscular power to fly, the following original, though perhaps questionable, way of treating the subject may be quoted (pages 25 to 28):—

"It would seem a waste of words to argue that a man can raise himself by his legs, without going up a ladder (which is in fact a flight in which support is taken from the rundles instead of from the air). No one can advance a step on level ground without lifting his entire weight. Each step (starting from the erect position) commences by a fall forwards, which is arrested by the advancing foot as it reaches the ground. Now, if there is a fall, it must be followed by an equal rise, which is effected by the leg that is left behind, which pushes his body forwards and upwards till the centre of gravity recovers its former height. The body is thus raised in walking chiefly by the muscles of the calf, extending the foot and opening the angle between the instep and the skin. The leg that did this part of the work, and was left behind, is then lifted by itself and brought forward to receive the next fall in its turn. The longer the step taken the greater the height through which the walker falls, and the greater therefore the height through which he must raise himself. I shall not overrate the height

through which the centre of gravity of the body falls and is lifted again, if I assume it at three inches for every complete step of a yard. If this be so, in walking a mile a man will have lifted himself through 3 + 1760 inches = 146·6 yards, so that his mile's work may be represented as equivalent, so far as his legs are concerned, to a flight directly upwards to a height of about 146 yards. . . . The enquiry now arises—Can the power thus available be applied to the air without such loss as to make it useless in practice? This I do not undertake to demonstrate, but shall leave the question to the advocates of mechanical flying. In attempting to solve this problem, however, provision must be made that, in case of an accident, the flyer shall not at once fall headlong."

Chapter 3, upon "the impossibility of propelling balloons, and the first difficulty in aërial navigation," is well handled. The following, from pages 34 and 35, will interest our readers:—

"The balloon has become a means of making a livelihood. which held out to needy men, generally innocent of science, a prospect of acquiring a competency, and perhaps wealth, with the addition of notoriety. While then they have been racing with each other up to the clouds for mammon or a maintenance, it was not likely that they could stop to consider whether it were possible to travel together upon a level course. What again could be done by isolated contrivers? One describes his device in a Journal or writes a Pamphlet; another criticises his plan, picks out some absurdity, and proposes a rival crotchet of his own, with which some one else finds fault in turn. One burdens himself and his scheme with letters patent. Another pompously declares he has solved the great problem, but will not make revelation thereof till he is well paid; and the men of capital who, each by himself, might be able to do but little to favor the growth of an useful art, however well disposed

to do so, are either unwilling to unite their means, except for the purpose of increasing them, or have been discouraged by the repeated failure of former individual schemes.

"But to return to propulsion. I have said that to propel the balloon is simply impossible. This has long been apparent to mechanical minds that did not happen to be enthusiastic about aëronautics, and has been pointed out over and over again, and the difficulty has been supposed to be an insurmountable barrier to any attempts to direct gas-vessels of any form. That which is an impossibility for the balloon is still a serious difficulty for gas-vessels of a more reasonable shape."

The above extracts will give some idea of the work, and we would strongly recommend those of our readers who contemplate making experiments to carefully study this very valuable contribution to the science of aërial navigation.

It is now an established fact that models of very different forms have been made to fly by means of a stored-up power contained within themselves, even when this power is employed for obtaining a support or abutment upon the air by very different modes of application. Those who will dogmatically assert that flight is impossible for man, may say that it is apparent that the power required and employed to support these models of lath and fabric is enormous for the weight, and scarcely affords any hope of being able to solve the problem; but then it can be argued that this power is not a fixed and definite one in all conditions. On the contrary, there is perhaps no application of power in which the extremes of much or little, are so widely different, according to the ways and means by which it is utilized. So little advance have we made in this problem, that perhaps in the very models that are now employed to demonstrate the possibility of a form of mechanical flight—and do so unmistakably—in reality effect their support and progress in air with the maximum expenditure of power;

were it otherwise not much scope would be left for improvements. The minimum of power requisite has yet to be arrived at, and even theory on this subject has not been sufficiently advanced. to the present date. to afford a solution of the question.

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The following SPECIFICATIONS OF PATENTS

ARE PRESENTED TO THE SOCIETY BY THE COMMISSIONERS.

Date.	*No.*	*Subject.*	*Patentee.*
1876.			
Mar. 7.	924.	Improvements in and appertaining to Machines for Aërial Navigation	H. BALLINI. J. W. PAYNER.
June 13.	2313.	Balloons for Aërial Navigation—Communicated by Count A. APRAXINE.	
Sept. 4.	3359.	Machinery for Propelling and Guiding Vessels on land and through air and water – Communicated by L. BRENNAN and W. CALVERT ...	H. J. HADDON.
Oct. 15.	3814.	An improved Military Apparatus or Aërial Battery—Communicated by A. W. GITTENS	C. O. ROGERS.
Oct. 27.	3974.	Improvements in Aëro-Navigation and in the construction and use of Aërostats, and in the Machinery and Apparatus therefor which improvements are in part applicable to other purposes....................	P. BRANNON.

BOOKS, PAMPHLETS, &c., RECEIVED.

Solution Complète de la Navigation Aërienne—By the AUTHOR, M. PERIGEUX.

Annual Report of the Board of Regents of the Smithsonian Institution, Washington, for 1875—By the REGENTS.

Angus and Mack on the Air Path; James Armour, C.E.—By the AUTHOR.

Catalogue of Special Loan Collection—By the COMMISSIONERS.

Angus and Mack on the Air Path, Part III; James Armour, C.E.—By the AUTHOR.

The Monthly Numbers of L'Aëronaute—By M. LE DOCTEUR DE VILLENEUVE.